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TRACTS

ON THE

NATURAL HISTORY

OF

ANIMALS AND VEGETABLES,

TRANSLATED FROM THE ORIGINAL ITALIAN OF

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WITH

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OBSERVATIONS AND EXPERIMENTS

ON THE

SEMINAL VERMICULI OF MAN AND OTHER ANIMALS, WITH AN EXAMINATION OF THE CELEBRATED THEORY OF ORGANIC MOLECULES.

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CHAP. IV.

REFLECTIONS ON M. DE BUFFON'S OBSERVATIONS—COMPARISON OF SEMINAL VERMICULI AND THE PUTREDINOUS ANIMALCULA OF SEMEN.

LET us continue to examine the other principal results from De Buffon's obſervations, that is, the appearance of thoſe very long and ſlender filaments which the vermiculi drag along in their courſe; the contraction and diſappearance of them; the activity the vermiculi acquire with age; the facility of motion in every direction; the change of form, diviſion, diminution of ſize; and, laſtly, their total deſtruction in a few days.

To begin with the firſt: Leeuwenhoeck, myſelf, and other naturaliſts, have remarked that each vermicule in the ſemen of man, and of ſeveral other animals, has a long appendage which it drags along as it ſwims. But this appendage is not as Buffon would incline, a thread or long

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corpuſculum, without analogy to a tail or any other member, and entirely foreign to the ſpermatic vermiculus. I have always found, as has been ſhewn in the firſt chapter, that the appendage has every characteriſtic of an actual tail. It has the ſhape, and the diameter always becomes greater on approaching the body to which it is united, ſo as to form one whole, as is ſeen in tailed worms: and the vermiculi alſo uſe the tail while ſwimming through the ſpermatic fluid, bending it firſt to one ſide, then to another, and in every different direction as aquatic worms are wont to do while they ſwim. I have ſeen this innumerable times, and in the moſt diſtinct manner; ſo that I muſt diſcredit my eyes, if I think or write differently.

It is true that microſcopic obſervation on the tail of human ſeminal vermiculi is the moſt nice and delicate of any I have made: it demands the greateſt care and the ſtricteſt attention. The tail is wonderfully ſlender, and at the ſame time tranſparent; whence, too ſtrong light confounds it with the ſeminal fluid, ſo that we entirely loſe ſight of the whole. In the firſt place, the choice of light, adapted for obſervation, is of the greateſt importance. The direct light of the ſun is too powerful, as is that of a lamp, unleſs it is moderated by ſome method. That which I have found moſt fit for this fine obſervation, is the light of a window

window oppoſite a white wall moderately illuminated: as, for example, when expoſed to a part of the ſky covered with white clouds. Secondly, the finer and thinner the ſliders are, on which the drop of ſemen is depoſited, the eaſier are the tails diſcovered: I prefer talc to chryſtal. Thirdly, the drop ſhould be as thin as poſſible, otherwiſe the origin of the tail will not be diſcerned; the reſt is concealed in the liquid; and when the ſeminal fluid is turbid, it is neceſſary to dilute it with a clearer portion. Fourthly, when the vermiculi ſwim, as the tail is always a little lower than the body, we muſt depreſs the focus of the lens to obſerve it. Finally, a microſcope of a ſingle lens, ſuch as that called Leeuwenhoeck's, ſhould poſitively be preferred to the compound microſcope.

Although M. de Buffon, in his obſervations, mentions only one or two of the precautions alluded to, and which he ſeems to have uſed, I am unwilling to think he neglected the reſt; for the exception of only one would prevent him from making the real obſervation. He ſays he always uſed the compound microſcope in examining the ſemen of man and animals. I doubt not that his microſcope was as excellent as he affirms; but it was a compound one, and had all the defects of compound microſcopes, among which is ſpecially placed, the object never being ſeen ſo diſtinct, or its

outlines ſo well defined, as with a perfect microſcope formed of a ſingle lens. This is a fact acknowledged by all obſervers, and my own obſervations on ſeminal vermiculi with both microſcopes confirm it more. Uſing the firſt, I ſaw the vermiculi preciſely as has been deſcribed: with the ſecond, I had a confuſed view of the body, and was frequently in doubt whether it was round or oval; the outline was always in a ſort of miſt or cloud. The tail, which is infinitely more delicate, appeared leſs ſenſible, and could only be diſtinguiſhed as a very long ſlender body. It is not ſurpriſing, therefore, that M. de Buffon calls this part of the vermicule a foreign ſubſtance, a kind of long, delicate, ſubtile filament, ſince it truly appeared ſuch to him when viewed with the compound microſcope.

It may perhaps be thought that my compound microſcope was not ſo good as M. de Buffon's; but thoſe I uſe are the moſt perfect now made in London. I was deſirous to view ſpermatic vermiculi with the microſcope M. de Buffon had employed, that is, Cuff's microſcope, which is preciſely that of Mr Needham, and which, M. de Buffon obſerves, he uſed in his examination of the ſeminal fluid. But it ſhowed me nothing more; and I may affirm that my obſervations and experiments on ſeminal vermiculi, as well as on the animalcula of infuſions, and ſimi-

lar

lar beings, could never have been exempted from uncertainty, I will even say error, by preferring the compound microscope to that of Leeuwenhoeck.

Let us look back a moment to establish the certainty of tails in the human seminal vermiculi. The fact is confirmed in so convincing a manner by the experiment on talc, where the tails appear complete, and not confounded with the semen, as to put it beyond all doubt was there no other proof. However, we may observe that such extreme circumspection, and so many precautions are unnecessary to distinguish the tail in the vermiculi of many animals, when Leeuwenhoeck's microscope is used.

To the contraction and entire disappearance of the tails, which Buffon says he observed when the vermiculi remained long in the semen after it came from the animals body, I can oppose nothing except that in all my observations, which have been innumerable, the reverse was uniformly seen. The vermiculi constantly preserve this member not only while alive, but long after death; and it does not begin to spoil or be destroyed till the vermiculi themselves arrive at the same state. Further, neither the maceration of the dead bodies by boiling, nor is freezing sufficient to destroy their structure or figure. Vinegar itself and urine, fluids which instantaneously

deſtroy the contexture of moſt infuſion animalcula, cannot, before a conſiderable time, affect the tail or body of our vermiculi. We ſhould, therefore, have to deny the exiſtence of all theſe facts to agree with M. de Buffon concerning the contraction and diſappearance of the tails. That my obſervations may have greater weight in eſtabliſhing the certainty and continuance of theſe tails, I ſhall cite the authority of the great phyſiologiſt of Berne: 'Nunc quod caudas attinet, 'perpetuam particulam vermiculi ſpermatici, eæ 'nimis certos, et fide digniſſimos habent teſtes, 'quibus meum ſuffragium per experimenta na-'tum addidiſſe liceat (1).'

The other phenomena of ſpermatic vermiculi, obſerved by M. de Buffon, ſuch as an increaſed activity acquired with time, their change of ſhape, diminution, diviſion and duration of life from four to eight days, ſeemed to me no leſs paradoxical, particularly on attending to the repeated obſervations mentioned in the firſt chapter, and recalling thoſe of the moſt acute Leeuwenhoeck. M. de Buffon's acknowledged merit at the ſame time prevented me from conſidering this aſſemblage of facts as chimerical; and unable to aſcribe it to the fault of his microſcopes alone, for, however defective they might be, they could not occaſion

(1) Haller, Phyſiologia, T. 7.

occaſion ſuch a difference in the phenomena, I determined to reſolve my doubts by taking the trouble to make a new courſe of experiments on human ſemen and that of other animals. But notwithſtanding all my care, precautions, and all poſſible vigilance, I could diſcover nothing new, at leaſt eſſentially affecting the facts I have ſpoken of. Yet, with reflection on Buffon's obſervations, and the repetition of my own, I could not reproach him with ſeeing what did not exiſt. I thought the whole might be an equivocal effect, which ſeemed the more likely, as the phenomena which he ſays he obſerved in ſeminal vermiculi might be occaſioned by beings of a very different nature. My experiments on infuſions ſuggeſted it. I had remarked, that there is no part of an animal infuſed that does not give exiſtence to a particular kind of animalcula. They are produced indifferently by the muſcles, brain, nerves, membranes, tendons, veins, and arteries; alſo by the blood, ſerum, milk, chyle, or ſaliva, mixed with water or even by themſelves. I had not yet made experiments on human ſemen for a ſimilar purpoſe, but it was moſt probable that the putrefaction of it would give exiſtence to particular beings: and who knows, ſaid I to myſelf, that they have not inadvertently been confounded with ſeminal vermiculi, and that M. de Buffon has aſcribed to the latter the properties and phenomena exhibited by

the former? Perusing Haller's Physiology again, I found he was of this opinion; nay, that he suspected M. de Buffon had never seen the spermatic vermiculi. 'Porro Buffonius, ut cum illustris viri venia dicam, omnino non videtur vermiculos feminales vidisse. Diuturnitas enim vitæ, quam suis corpusculis tribuit, ostendit non esse nostra animalcula (id est spermatica) quibus brevis, et paucarum horarum vita est.' And in another part of the same volume, he adds, 'Ea enim experimenta (id est Buffonii) manifesto duc-cunt ad animalia putredinosa (1).' However, I thought of ascertaining this fact, by observing what successively happened to semen long preserved in a watch-glass.

My first experiments were on the human seminal fluid (2). The vermiculi died in three hours and a half, and were precipitated to the bottom of the glass. On the sixth day, the fluid began to exhale a fœtid disagreeable odour: but no animated being was perceptible. The bodies of the vermiculi were seen, apparently very entire, in some drops taken from the bottom of the glass. The seventh and eighth days, there was no change; but the fœtor increased. On the ninth, I discovered very minute animalcula, their

(1) Tom. 7.

(2) 11 February, The Thermometer 45°.

their ſize nearly equalling that of ſpermatic vermiculi; but they had no tail, and much reſembled moſt minute ſpherules. Like infuſion animalcula, they ſometimes ſtopped at little fragments of corrupted ſemen: ſometimes their courſe was very rapid, retrograde or riſing and ſinking in the fluid: In a word, they poſſeſſed every property of animalcula. They were ſeen in every ſtratum of the ſemen, and thoſe at the bottom moved the dead bodies of vermiculi, which were ſtill entire, and remained ſo ſome days longer. In the ſeminal animalcula, we remarked the ſame periods with regard to increaſe, diminution of numbers, and termination, as is generally obſerved in the hiſtory of other animalcula; only, when the globular animalcula decreaſed, there came two other ſmaller generations: the laſt, which could ſcarcely be diſcerned, continued until the eighteenth day.

While examining the ſemen in this glaſs, I obſerved a portion of the ſame kind that had been put in another glaſs, and placed in a ſtove, that it might experience a greater degree of heat (1). The effect was ſuch as we might expect. The vermiculi lived longer, and the infuſion animalcula appeared ſooner: the former died in five hours, and the latter appeared in five days. They were

(1) The thermometer at temperate.

were of a globular figure, had no tail, and all of the same size and species as those of the preceding observation. The other two colonies then appeared, the globular always remaining.

Having again prepared the same kind of semen, I had an opportunity of seeing the effect of heat in accelerating the production of animalcula (1). Some were found in the fluid in less than twenty-three hours after it was taken from the dead body. These were of a different species from the globular; they were three times as large, and of a cylindrical figure. The body in swimming undulated like a serpent, which did not occur, or was unobserved, in the globular animalcula. The dead vermiculi seemed to be their food; because the animalcula incessantly in motion surrounded them, and pecked at them with the anterior part of the body. In three days, other animalcula as minute as the spermatic vermiculi appeared, along with the cylindrical; and I remarked a circumstance which in semen I had not observed before. In the Tract on infusion animalcula, the propagation of many species by natural division has been spoken of at large. It has been said, that, in several, the division begins in such a manner that the animalcule is gradually cleft across, until it is divided into two equal parts,

(1) 22 May, The thermometer 65°.

parts, which become two animals ſmaller than the firſt: but we had to learn whether this mode of propagation could happen with animalcula ſimilar in ſize to ſeminal vermiculi. Several ſcores divided tranſverſely before my eyes; and the diviſion or propagation continuing ſeveral days, the ſeminal fluid, which had become exceſſively fœtid, now teemed with life. But the number of theſe, as well as of the cylindrical animalcula, gradually diminiſhed, in the ſame way as the numbers of infuſion animalcula decreaſe; and on the twenty firſt day, all had diſappeared. There only remained an univerſal obſcure fermentation of the ſemen in no particular direction, but the ſeminal molecules were tumultuouſly driven about to every ſide. I was not long in perceiving that this irregular agitation was occaſioned by a multitude of moſt minute animalcula concealed in the ſemen, which their courſe had put in motion; and of this there was complete evidence by diluting the ſemen with water, as they were then accurately diſcerned, appearing about half the ſize of vermiculi.

The phenomena diſcovered in the ſemen of the horſe were analogous to thoſe in the human ſemen (1). The vermiculi lived ſeven hours, and then were precipitated to the bottom, where they remained

(1) 26 July, The thermometer 88°.

remained long entire; the body and tail were complete for a month. In fourteen hours, the ſemen began to exhibit ſymptoms of putridity, and then were infuſion animalcula viſible; they increaſed; and on the fifth day, there were many ſpecies. One particularly demands our notice: not only did it multiply by longitudinal diviſion, but the ſize and figure changed every moment. Sometimes the animalcula contracted and became round; ſometimes they dilated and became elliptical, as I have remarked in ſeveral ſpecies of infuſion animalcula.

When this experiment on the ſemen of the horſe was made, one ſimilar was made on that of the rabbit. The vermiculi periſhed in four hours, and fell to the bottom; animalcula appeared in fifteen. Two ſpecies multiplied by diviſion; and one exhibited the contractions and inflations already deſcribed.

The ſame experiments were made on the ſeminal fluid of the ram, dog, and bull; on that of frogs and newts, with analogous reſults. In the beginning, and during the progreſs of putrefaction, each produced different animalcula, and all diſplayed numerous and different ſingularities. Their number increaſed; it diminiſhed, and became very ſmall. The animalcula were different in figure and ſize, and ſome ſpecies multiplied by diviſion; which proved that animal ſemen reſembled

ſembled vegetable ſeeds, from the many kinds of beings to which it gave exiſtence.

Theſe facts afforded new light in illuſtrating how erroneouſly Buffon had aſcribed to ſeminal vermiculi properties that belong to animalcula of infuſions only. Let us collect the circumſtances in a few words.—According to him, the vermiculi, after a certain time, were deprived of their tails. He ſhould have ſaid, the animalcula of infuſions came in the place of the vermiculi, which were already dead and precipitated to the bottom of the liquid. He was arreſted by their firſt appearance, and took them for ſeminal vermiculi deprived of the tail, which in truth they often very much reſemble.—When diſengaged from the tail, he adds, they acquired much greater activity. This was a neceſſary conſequence of the firſt miſtake. When the animalcula had come, their increaſed velocity could not be unobſerved, ſince they move with much greater rapidity than vermiculi; and the erroneous ſuppoſition being admitted, Buffon had to relate, as he has done, the remainder of the phenomenon. He had to ſpeak of the imaginary changes of the vermiculi, of their diviſion and diminution, with the more confidence, as his opinions were confirmed by a repetition of his experiments, if not on all, at leaſt on many ſpecies of infuſion animalcula in the ſemen.

I think

I think I have had too numerous and too decisive proofs to doubt that the phenomena seen in infusion animalcula of semen are totally different from the phenomena exhibited by seminal vermiculi. May I not at once be permitted to oppose my experiments to those of the celebrated De Buffon? I observe that all the seminal fluids he has studied, and where he has discovered the phenomena of which we have spoken, have also been studied by me. I perceive that even more have been examined, and those both of cold and warm blooded animals. His observations on seminal fluids were only made at certain times, and in one season. I judged it necessary to extend mine to all seasons. My microscopes were not inferior to his, nay, they were much better. How then is it possible, that in the same circumstances with the French naturalist, and even in situations more advantageous for examining the phenomena of seminal vermiculi; how is it possible, I say, that these phenomena were never observed? that I never observed some, or remarked a single one? —What do I say? Not only have I never seen any of the phenomena, but beheld what completely contradicted M. De Buffon's observations, in all the fluids, many, many times examined. Such for example is the imaginary activity acquired by vermiculi, in proportion to the time they remain in semen; since directly the reverse happens, as

has

has been obſerved and is mentioned in the firſt chapter. For after expoſure to the air, their motion, which before was quick, becomes much more languid; and its greateſt rapidity is when the ſemen comes from the animal's body, which, as I have already ſaid, was remarked by Leeuwenhoeck.

Buffon ſays, he obſerved the phenomena three or four days after the ſemen came from the animal, even on the eighth day in that of the rabbit. This muſt be impoſſible, becauſe the vermiculi living longeſt, as thoſe of the human ſemen, which were examined by M. de Buffon, cannot exiſt above ſeven or eight hours in the open air. When ſecured againſt the influence of air, in glaſs tubes hermetically ſealed, they do not ſurvive three days, as we ſhall afterwards ſee. Thus it is certain, that the phenomena which the author obſerved in his experiments could be exhibited by none but the animalcula originating in the ſeminal fluids when on the verge of corruption, or when they do corrupt, as ſucceeds in other liquids that will corrupt, or are already corrupted.

I cannot ſuppreſs my ſurpriſe, that the celebrated French author never doubted, whether the animated beings which he beheld in ſemen were really ſpermatic vermiculi, or only animalcula originating there, that is, infuſion animalcula. Theſe animals, he well knew, originate no

leſs

leſs in animal than in vegetable infuſions when beginning to corrupt; for he ſays, that in two infuſions, one made with the teſticles of a ram in water, and the other with thoſe of a dog, he, in ſome days, found living animals ſimilar to what he had ſeen in the ſemen of animals: that is, globular or ovular, without tails, moving with great activity, and often changing their ſhape(1). If the animalcula of the two infuſions were exactly ſimilar to thoſe he had obſerved in ſeminal fluids, how did he not ſuſpect, that inſtead of being ſpermatic vermiculi, they might be infuſion animalcula? He had additional reaſon to think ſo, for he muſt have remarked that the changes of figure, diviſions, diminutions of ſize, were not to be ſeen in recent ſemen, but in that kept ſome time, and about to corrupt. Of this he muſt have had certain indications, and an indubitable proof, from the fœtid and cadaverous odour which the ſemen then exhales, which alſo, is convincing evidence that animalcula are produced in it on account of its putreſcence; conſequently that we cannot confound them with ſeminal vermiculi. Another precaution might have occurred, and ſaved him from error, had he choſe to adopt it, namely, to examine the bottom of the ſeminal fluid and not the ſurface alone. There he would have found

(1) Hiſtoir. Natur. tom. 3.

found the vermiculi entire though dead, which would have demonstrated, that the animals he then saw in motion could not be seminal vermiculi.

But all I have hitherto said receives greater weight from the comparison that has been made between the vermiculi and animalcula of putrid semen. In another work, I have shewn that a considerable part of infusion animalcula appear by the microscope an aggregate of minute vesicles, of different sizes, and more or fewer invested by a common pellicle, forming the exterior of the animal; that the pellicle and its vesicles are lost and destroyed when the animalcula die; and if, while alive, they are wet with urine or vinegar, the body is destroyed and reduced to nothing(1). All these things are amply verified in the putredinous animalcula of semen; but, with the utmost care and attention, I have never been able to see any thing like it in seminal vermiculi. The texture of the body and tail is not vascular; it is uniformly homogeneous, connected, equally solid, and compact. For this reason perhaps the dead vermiculi fall to the bottom of the seminal fluid, and the infusion animalcula commonly swim. The vermiculi likewise continue long entire after death; urine, vinegar, even boiling, cannot dissolve or decompose their substance.

(1) Saggio di Osservazioni Microscopiche.

From all this, and from all that has already been ſaid, we muſt conclude that the animalcula of infuſions, conſequently thoſe of putrid ſemen, are of a nature and conſtitution eſſentially different from ſeminal vermiculi. That it is truly ſo we may eaſily determine, ſince a fluid which affords a ſalutary dwelling to the one is fatal to the other. Putredinous ſeminal animalcula originate and live in corrupted ſemen, but die in that which is recent and entire. On the other hand, vermiculi live with ſafety in recent, but periſh when put in corrupted, ſemen. The animalcula become more lively and active when water is mixed with the ſemen; the vermiculi, at leaſt ſeveral ſpecies, become motionleſs and die. Of the whole of theſe facts I have often convinced myſelf: Whence, I conclude infuſion animalcula and ſpermatic vermiculi to be two different ſpecies of animals which we cannot confound without outraging nature.

CHAP.

CHAP. V.

DEDUCTIONS FROM THE FACTS STATED.—M. DE BUFFON'S OBJECTIONS ANSWERED.

As I have taken the liberty reſpectfully to remark the errors of M. de Buffon, I muſt ſtill requeſt permiſſion to ſhow the deductions that may be made from his noted theory of organic molecules. But to do this with the greater ſucceſs, it will be neceſſary to bring under our view ſome of the chief points of that theory.

The illuſtrious French naturaliſt ſuppoſes that every animal and vegetable ſubſtance includes a number of organic molecules, that is, active and incorruptible particles. He thinks they ſerve for the nutriment, increaſe, and multiplication of all beings, whether entering the body of animals by means of food, or the ſubſtance of plants with the juices they imbibe; that they intimately penetrate every part, unite there, and are indentified, if we may ſay ſo, and afford nouriſhment to the plant or animal. If either is young, it appropriates all the organic molecules, incorporat-

ing them with itſelf; and, by extending and expanding the fibre, they effect increment. If the being is already an adult, if it no longer is ſuſceptible of expanſion, then all the molecules being unemployed in nutrition, thoſe which are ſuperfluous are depoſited in the organs of generation, and ſome appropriated for propagation of the ſpecies; an event which ſucceeds when the organic molecules of the male are mixed with thoſe of the female in the matrix, ſo that the moſt analogous tend to approach each other, and, uniting in virtue of certain relations, form individual wholes, reſembling, as in miniature, the different parts of the two individuals in which they are modelled; from all theſe individual wholes together, there reſults a general whole, which is the embryo.

If the organic molecules afforded by the male are more numerous than thoſe afforded by the female, or more active, the embryo will be a male; if the molecules of the female are more numerous, it will be a female. The greater number derived from either will occaſion the greater reſemblance in the embryo to the individual from which it has received them.

Large animals are leſs fruitful than ſmall; the reaſon is evident. The former extract fewer organic molecules than the latter. A bull draws leſs nutriment from hay and ſtraw, and conſequently

quently fewer organic molecules than a bee does, in proportion, from the finer parts of flowers.

Animals covered with ſcales are more productive than thoſe covered with hair, probably becauſe, by perſpiring leſs, they accumulate a greater number of organic molecules.

If, inſtead of collecting in the organs of generation, the molecules are carried to other parts of the animal, they there form minute living animals, as *teniæ* and *aſcarides*, worms ſometimes inhabiting the inteſtines, the liver, and the ſinoſities of the brain.

Thus does the theory of M. de Buffon explain theſe phenomena, and ſome others, which, for the ſake of brevity, I ſhall omit. Wiſhing this theory, the offspring of his fertile genius, to be adopted by nature, he recurs to the ſeminal fluids of animals and the infuſions of plants, becauſe in both, according to his opinion, are organic molecules clearly exhibited under the form of globular, ovular, or other ſhaped corpuſcula, endowed with motion, ſubject to various changes of figure, dividing into ſeveral ſmall bodies, and then acquiring greater activity, which augments more and more, as they are further decompoſed, until their minuteneſs renders them inviſible.

This laſt trait of M. de Buffon's theory proves that it reſts entirely on the facts related by

its author, that is, on a falſe hypotheſis. For, with reſpect to infuſions, we have ſeen that there is nothing in them to indicate organic molecules, as the moving corpuſcula there are actually animals, ſome viviparous, others oviparous, and as thoſe, multiplying by diviſion, do not produce that progreſſion of ſucceſſive diminution which Buffon imagined he ſaw, but the ſmalleſt grow larger like other animals.

Having found the living putredinous beings of ſemen to be exactly of the ſame kind as thoſe of infuſions, by a direct and concluſive conſequence it follows, that they could never be confounded with organic molecules; and we may ſay the ſame of ſeminal vermiculi, whoſe animality has been ſufficiently proved by the facts related in this treatiſe, and thoſe engroſſed in the ſubſequent chapter.

M. de Buffon's theory is thus completely deſtroyed. Such is too often the fate of the hypotheſes of ardent and fanciful philoſophers, firſt invented and then ſought for in nature. This ingenious naturaliſt, diſſatisfied with the theories of generation already framed, and hurried on by his taſte for novelty, imagines an animated matter in bodies, original, incorruptible, and always active, which he ſpecioufly denominates *organic molecules*. Making them act according to certain terms, and with a certain effect, he thinks he

he can explain the great work of generation, and the moſt inſcrutable phenomena it preſents, and employs all that powerful and perſuaſive eloquence which characteriſes him as the orator of the age. Prejudiced in favour of his theory, it was not difficult for him to find it in nature. His views were leſs directed to ſee what actually exiſted than to what he wiſhed to find; not otherwiſe than his celebrated countryman, the reformer of botany, who fancied that metals and ſtones vegetated, and thought he had evidence of this imaginary vegetation, that he ſaw ſeeds and plants where there were none. If this learned academician, who has ever poſſeſſed my full eſteem, will take the trouble to repeat his experiments on the ſemen of man and animals, with better microſcopes, and, forgetting his favourite organic molecules, impoſe a rule on himſelf to receive as truth nothing but the images tranſmitted by the ſenſes, without adding the corrections of imagination, as is the duty of a veracious naturaliſt; I can aſſure him he will find all that I have witneſſed a thouſand times, and deſcribed ſo largely in my works. I earneſtly entreat him not to reject this without a trial, which muſt certainly reſult to the advantage of truth.

I now mean to examine Buffon's objections intended to prove that the living beings ſeen in ſpermatic fluids are not real animals. Some of them

have already been anſwered in the beginning of chapter 3; and it appears, from what I have hitherto ſaid, that ſome are falſe; ſuch as, the formation of the animals under the obſerver's eye; the loſs of their tails, and diminution of ſize. Two yet remain to be ſpoken of; their diviſion into parts, and their frequent change of ſhape. Theſe phenomena are real, though never obſerved in ſeminal vermiculi, and only in the animalcula of infuſions. M. de Buffon eſteems them incompatible with the character of animality.

With regard to the objection concerning diviſion, it is needleſs to ſtop here for a diſcuſſion; and I paſs to the ſecond relative to the metamorphoſes of putredinous animalcula. It is true that the body of the animals familiar to us is ſo conſtructed that the ſhape never varies, and cannot be materially altered without deſtruction; but it is no leſs true, that there are others experiencing the contrary; as for example, among inſects, many ſpecies of worms. To be convinced of this, it is only neceſſary to glance at the operations of Nature, or to open the works of naturaliſts. How many worms of this kind have been elegantly deſcribed by Redi and Valliſneri! A ſingle ſpecies may ſerve for many, and this is the *cucurbit* worm. Is it not certain by the obſervations of theſe two, eſpecially of the Tuſcan philoſopher, that they aſſume numerous and various

rious ſhapes, and, to uſe his own words, 'Some-'times they contract and ſwell into purſes, ſome-'times the extend and curve into a ſemicircle.' Does not Reaumur ſay the ſame of certain worms changing into flies, whoſe head, that part conſtantly the ſame in moſt animals, in this inſect ſo wonderfully changes its appearance as to be ſometimes extended, depreſſed, or contracted; ſometimes obtuſe in one part, and acute in another. Do not we daily ſee the ſame changes in earth-worms, ſnails, and particularly in leeches, extending the body till it is long and ſlender, then contracting till it becomes ſhort and corpulent, or growing thick at one end and ſmall at the other? What ſhall we ſay of the wheel animal, that aquatic creature, which, from its wonderful metamorphoſes, we may term the Proteus of the inſect tribe? If wheel animals, earth-worms, leeches, ſhell and naked ſnails, and ſo many ſpecies of worms (1), are not degraded from the rank of other animals, notwithſtanding their different mutations of figure, the ſame ſhould be the caſe with the living beings in putrid ſemen (2).

Two

(1) The author does not mean that theſe animals belong to the claſs of vermes.—T.

(2) Notwithſtanding this, Leeuwenhoeck obſerves, 'Although ſeminal animalcula are ſhaped like an eel, they can 'contract their bodies ſo much as to become round.'—T.

Two other objections are ſtill to be appretiated; one deduced from the ſingular motion of ſeminal vermiculi; the other from the different effects of heat and cold upon them. Let us attend to the firſt. An animal, according to M. de Buffon's reaſoning, is ſubject to change its inclinations. Sometimes its motion is quick, ſometimes ſlow, then it ſtops and reſts. But nothing of this occurs in ſeminal vermiculi: they are in continual motion, never reſt, and, when once they ſtop, it is for ever. Hence they cannot be animals. This objection is, like the former, both being founded on the analogy of the large animals beſt known to us; which, as they do not change their ſhape, naturally have the viciſſitudes of reſt or motion, of accelerated motion, or the like. But to admit that ſuch viciſſitudes are certainly a characteriſtic quality of animals, it is improper to conſider theſe alone; it is neceſſary to examine other genera and other ſpecies, and to dwell particularly on the ſmalleſt, eſpecially thoſe inhabiting fluids which have more analogy to ſpermatic vermiculi. It is undoubted, that among them M. de Buffon would have found ſeveral animals, which, far from having the alternatives of motion and reſt, are naturally in motion; ſo that their life ſeems to conſiſt in a perpetual motion. To aſcertain the fact, it is neceſſary, during ſummer, to obſerve the water of marſhes,

marſhes, ponds, and ditches, where all inſects are. There we ſhall ſee ſome in motion by conſtant contorſions; as, for example, the worms mentioned by M. Trembley, ſerving polypi for food: the body is continually in oſcillation (1). Without the trouble of ſeeking them in the country, M. de Buffon may at his leiſure obſerve it at home in the eels or ſerpentuli generally found among vinegar. If a ſmall portion of this liquid is put in a thin glaſs veſſel, and expoſed to the light when the ſun ſhines bright, on examining the higher parts with a magnifier, where the eels are more diſtinctly ſeen, their contorſions and undulations appear inceſſant; they dart from one ſide to another, and continue thus from morning to night: in this manner do they perſevere ſeveral months, that is, to the end of their lives. It would ſeem, from theſe facts, that the perpetual motion of ſeminal vermiculi cannot be a ſufficient reaſon for proving they are not animals. But, farther, ſuch motion is not natural to the vermiculi; it is forced and violent; for they have quitted their native abode to enter our atmoſphere; they experience the lively impreſſion of the air, which hurts and injures them, and conſtrains them to perpetual flight. Doubtleſs the air is noxious, and occaſions their continual motion,

(1) Memoires ſur les polypes.

tion, as is proved by facts. To fix the point, I waited till the human ſemen cooled. I then took ſome drops, and ſpread them very thin on a watch-glaſs. The vermiculi of the ſpread drops, though theſe were of conſiderable thickneſs, conſtantly periſhed ſooner than thoſe of the whole ſemen; becauſe, as I think, the former were more expoſed to the air. Two equal portions were taken at the ſame time, and the one put in a cloſe and the other in an open veſſel: the vermiculi of the latter always died ſooner. The privation they underwent clearly aſcertains how injurious this element was to them. They lived longer in vacuo than in the open air; ſo that all were dead, when ſome ſtill lived in a vacuum. The difference of time in their death was an hour and a half, two, even three hours, and ſometimes longer, according to the ſeaſon of the experiments. Theſe facts prove, that the air is noxious to the vermiculi; and the following evince that it is the cauſe of their being in continual agitation. Capillary tubes were made at the blow-pipe, and one end immerſed in recent ſemen; it aſcended the cavity of the tube to a conſiderable height. Breaking the tubes near the part to which it roſe, I preſented this extremity to the blow-pipe, and inſtantly ſealed it. The ſame was done to the other end, by which means the ſeminal fluid was deprived of all communication with the external air.

air. The tubes were ſo drawn out, that the thinneſs of the glaſs permitted me to ſee the vermiculi within. The peculiarities they preſented were very different from thoſe of the reſt. All, or at leaſt moſt of them, had a ſingular mode of moving; ſome had that kind of motion obſerved in thoſe expoſed to the open air; others had a continual irregular motion, changing from velocity to inaction and reciprocally; others ſtopped entirely, and, after reſting ſeveral minutes, reſumed their former rapidity: we did not ſee them inconſiderately ſtrike againſt the ſolid portions of the ſemen, as was remarked in the firſt chapter, but always avoiding them and turning aſide or retreating. Theſe peculiarities indeed ſucceeded better, and with more uniformity, when the tubes were kept warm. I have before ſaid, the longeſt period of life of the human vermiculi is ſeven or eight hours when expoſed to the open air; but how much is it prolonged when they are included in tubes? In ſummer I have ſucceeded in preſerving them two days or more, and in ſpring and autumn almoſt three (1).

It

(1) Authors diſpute very much concerning the duration of the life of theſe animals. Some have ſuppoſed they live only a few hours, and others, ſeveral days. Leeuwenhoeck is a moſt exact obſerver; he ſays, he put the ſemen

It will undoubtedly ſeem paradoxical, that the vermiculi live longer in ſpring and autumn than in ſummer. The reverſe apparently ſhould happen; for the heat of ſummer ſhould be more congenial to them, as it approaches nearer to the natural heat of living man. At firſt, it gave me conſiderable ſurpriſe, nor was it diminiſhed on reflecting, that in open air they live much longer when the weather is warm. This induced me to repeat my experiments. And I conſtantly found, that, during ſummer, they never lived as long in tubes as during ſpring and autumn. In ſummer, they abſolutely die ſooner on the warmeſt days. With a little reflection, it is not difficult to comprehend the cauſe of the difference. We have ſeen that the ſemen of man and animals, when removed from its natural ſituation, very ſoon becomes putrid, and putrefaction takes place earlier as the heat is greater to which it is expoſed. To this cauſe, therefore, I aſcribe the more immediate

men of a dog in a glaſs tube during ſummer; many *animalcula* were dead the firſt day, more on the ſecond and third, and on the fourth very few were alive. Next year, he examined ſome ſemen taken from the ſame dog; for ſeven days and nights, ſome *animalcula* were ſtill alive, and a few ſwam with as much velocity as if they had recently come from the animal. *De diuturna vita animalculorum in ſemine maſculo canis.*—T.

immediate death of vermiculi in capillary tubes during ſummer. Having filled ſimilar tubes with recent ſemen, and ſealed them hermetically, I expoſed ſome to the heat of the atmoſphere at about 63°, and others to the heat of the human body, keeping them in the axilla in a large glaſs tube. In a ſtate of health, my own heat is about 97°. The vermiculi expoſed to the heat of the atmoſphere lived two days and a half; ſome even three; but thoſe experiencing the heat of my body ſupported it only thirteen hours. Their more immediate death cannot be aſcribed to the greater degree of heat, ſince it is about the ſame as what preſerves them alive, or in which they generally live: Nor can we aſcribe it to any other noxious principle ariſing from the nature of the tubes, ſince they were perfectly ſimilar to thoſe expoſed to the atmoſpheric temperature. It therefore becomes neceſſary to recur to ſome alteration or noxious quality contracted by the ſemen, which makes heat accelerate their death: and I acknowledge, that I cannot find this alteration or injurious quality but in the principle of putrefaction, which is manifeſted by the fœtor on breaking the tubes: certainly it is this that muſt be fatal to the vermiculi, as I ſhall demonſtrate in Chapter 6. The putrefying principle does not commence in the ſemen expoſed to the open air only ſeven or eight hours in ſummer.

On

On the other hand, as the heat of ſummer nearer than that of any other ſeaſon approaches what the vermiculi experience in us, we clearly ſee why they then live longer in the open air than at any other time: and for the ſame reaſon we may underſtand why life is abridged in proportion as the cold increaſes.

But it is time to come to the objection of heat and cold which the author thus propoſes. On expoſing the ſemen to the cold air, the vermiculi did not ſeem to ſuffer from it. They continued moving with their uſual quickneſs as long as thoſe not expoſed, though the fluid had acquired that degree of cold in water on the point of freezing, as one might be convinced by touching it. On the contrary, if the ſame vermiculi ſuffer heat, their motion ceaſes, although the heat is moderate. In conſequence of theſe facts, if the vermiculi are real animals (I am relating Buffon's reaſoning) they will exhibit an appearance and conſtitution very different from the appearance and conſtitution of other animals; as too much cold relaxes and deſtroys motion, whereas mild and moderate heat preſerves it.

We muſt regret that our author, inſtead of uſing taction to judge of heat or cold, did not employ a thermometer; for all philoſophers know the touch is a very equivocal proof. He ought to have diſcovered preciſely at what degree of

of heat the motion of vermiculi ceaſed, and the degree of cold by which it is not relaxed. I have eſteemed it moſt eſſential to ſupply this defect, in order to obtain arguments more concluſive and deciſive, and to learn if, with reſpect to heat and cold, the vermiculi are of a nature and conſtitution much different, as M. de Buffon imagines, from the nature of other animals.

Though the obſervations in Chapter I. do not ſeem to beſtow that vigour of conſtitution ſufficient to enable them to reſiſt cold, ſince it ſeemed their motion decreaſed with the decreaſing heat of the atmoſphere, ſo that when the thermometer ſtood at 36°, they continued moving only an hour and a half: yet I reſumed my experiments, reſolving to extend them further, and ſubject the ſemen to a degree of cold equal to freezing, and carefully obſerve what ſhould happen to the vermiculi. The ſemen firſt uſed was that of the horſe. On the 14 of January, it was expoſed to freezing: the vermiculi could not be more vivacious; their vivacity evidently diminiſhed; and in ſixteen minutes all were motionleſs, though the ſemen was not frozen. The cold becoming more intenſe on the 18, I repeated the experiment. The vermiculi became motionleſs in eleven minutes. The thermometer ſtood at 8° below freezing, but the ſemen was ſtill fluid. The experiments were often

repeated during winter. They conſtantly proved, that the duration of motion was proportioned to the temperature of the atmoſphere.

In proſecuting my experiments on the ſemen of the horſe in ſummer, a thought occurred of ſubjecting vermiculi to the cold of freezing, by immerſing the glaſs that contained them in ſnow. The ſame effect was produced as by the winter's cold, that is, in fourteen minutes, it made them motionleſs; though, when expoſed in another glaſs to the heat of the atmoſphere, they moved ſeven hours and a half. An accident that happened in the ſummer experiments afforded new intelligence, and diveſted me of a prejudice. Obſerving that the vermiculi had become motionleſs, I took the glaſs from the ſnow, and left it expoſed to the air at 81°. An hour after, it aſtoniſhed me to find all the vermiculi reanimated in ſuch a manner as if they had juſt come from the ſeminal veſſels. I then perceived the cold had not been fatal, but had reduced them to a ſtate of complete inaction. They were replaced among the ſnow, and in three quarters of an hour being taken out, I obſerved theſe phenomena: In a few minutes, their vivacity relaxed, and the diminution increaſed until they loſt the motion of progreſſion, and retained that of oſcillation only, which alſo ended in a few minutes more. Exactly the reverſe was ſeen, when they paſſed from the

the cold of ſnow to the heat of the atmoſphere. The firſt motion that appeared was oſcillation; the body and tail began a languid vibration from right to left, and reciprocally; motion was then communicated to the whole vermiculus. At firſt, it was ſcarcely perceptible; it ſoon increaſed, and grew very conſiderable. It ſhould likewiſe be added, as cold does not render all the vermiculi motionleſs at the ſame moment, but ſome later than others, neither does heat affect all with equal power.

I ſubjected the ſeminal fluids of man and the bull to the ſame experiment, and had the ſame reſults as from the ſemen of the horſe, except that a degree of cold leſs than freezing immediately deſtroyed all motion in the vermiculi of the bull.

On approach of the following winter, the ſame experiments were reſumed; and I ſucceeded in reanimating torpid vermiculi by breathing on the ſemen, by applying my finger to the tube on which ſome drops were put, or by placing it near the fire. Removed from this reviving heat, they fell into the ſame lethargy as when in ſummer they were tranſmitted from the atmoſpherical temperature to the cold of ſnow. During the rigorous ſeaſon, I expoſed them to a more ſevere trial: they were expoſed to cold above 9° below freezing. As might have been expected,

it immediately rendered them motionleſs. In five minutes, not a ſingle vermicule moved. When they had been expoſed five minutes longer, they were tranſported into warm air, and left there for ſome time. Although this intenſe cold continued ten minutes, the ſemen was not frozen, but it had fatally injured a complete third of its inhabitants. They evinced no ſign of life when removed to, and kept long in, a warm ſituation; on the contrary, they had all the appearance of death. The other vermiculi recovered indeed, but their motion was languid in compariſon to what it was before. The experiment was made 15 December, and repeated on the evening of January 5 at 10° under freezing.—In about a quarter of an hour, I perceived the ſemen begin to congeal about the edges of the glaſs; I then put it in a ſtove, but it was an ineffectual remedy for the vermiculi. Not one recovered; and thoſe environed by the ice periſhed as well as thoſe in the fluid parts. The like happened to the vermiculi of other two glaſſes, on which I made the ſame experiments this evening, notwithſtanding care was taken to regulate the different degrees of heat, leſt too ſudden tranſition from heat to cold might be injurious.

Such were the experiments with cold: any one may draw the concluſions. Very far from excluding vermiculi from the rank of animals, it

ſurpriſingly

furprifingly confirms them in it. For what can more fatisfactorily prove it than feeing languor become more immediate as the cold is more intenfe; feeing the vermiculi revive when brought to heat; and to witnefs their actual death, when the cold is of a greater degree? Such is the ftate of moft fmall animals, deprived of action and rendered torpid by cold; with heat, they recover life and motion, and yield under cold ftill more intenfe.

How can thefe facts, multiplied, repeated, uniform, confequently certain and incontrovertible, fubfift with the affertions of Buffon, who fuppofes cold does not impede the motion of feminal vermiculi? Inftead of negativing this illuftrious Frenchman's affirmation, I think there is a method of conciliating our obfervations. We have already remarked the error which occafioned his confounding vermiculi with animalcula, and afcribing to the former the properties pertaining to the latter only. It is very likely, that the effect of cold he has obferved on vermiculi is alfo a confequence of the fame miftake; and this is the more probable as it is feen in putredinous feminal animalcula. Nor do the animalcula of infufions alone, at leaft many fpecies, withftand cold of a great degree; for thofe found in putrid femen are undoubtedly of that number. Several experiments convince me of it; but to avoid the

ennui of my reader, I ſhall not detail them. There is one circumſtance which ought not to be paſſed in ſilence; that although theſe animalcula can ſupport more cold than ſeminal vermiculi, their motion does become languid; and, on increaſing it conſiderably, they periſh like inſects which yield to the greateſt degree of cold.

When I found theſe methods of conciliating Buffon's experiments and my own, with regard to the phenomena from cold, I attempted to find the ſame reſpecting thoſe exhibited by heat, but that was impoſſible. My obſervations have been directly oppoſite to his. Thoſe he made are comprehended in a few words: "The motion of "vermiculi ceaſes when they are expoſed to a "ſmall degree of heat." I entreat the reader to examine mine, that he may be enabled to compare and form an opinion.

Two watch-glaſſes were put on water contained in a veſſel, one containing a portion of recent ſemen, full of vermiculi; the other, an equal quantity of ſemen, old and ſwarming with putredinous animalcula. To know the ſucceſſive degrees of heat, I had put the ball of a thermometer into each glaſs. The water was gradually heated on a ſlow fire. As the thermometers roſe, I took ſome drops of ſemen from the glaſſes for examination with the microſcope. The putredinous animalcula were very vivacious at 99°; at 104°, their

their motion began to grow languid; at 106° and 108°, all perifhed. Seminal vermiculi are of a more hardy conftitution: they were active at 111°; fome began to periſh at 120°; and at 131°, there was not one alive: fo that the difference which occafions the deftruction of the one and the other is about 22 degrees. The vermiculi were thofe of human femen. The fame experiments were made on the femen of the horfe, the bull, and the dog. There was fome little difference: thofe of the horfe and dog periſhed at 126°, thofe of the bull at 133°.

A number of capillary tubes were next filled with femen, partly full of feminal vermiculi, and partly of putredinous feminal animalcula. I fealed them hermetically, and put them at the bottom of a veſſel of water, which was gradually warmed. A thermometer was alfo immerfed. When the water had attained 99° of heat, I began to examine the tubes one after another, as the heat increafed. The feminal vermiculi of man, and the other animals, died at 122° and 124°; and the putredinous animalcula at 106° and 108°.

Thefe facts demonftrate, that animalcula, originating in putrid femen, are of a conftitution better calculated to refift heat, which many other animals fupport, and which die only at the fame degree that animalcula from putrid fubftances

die, or nearly about it. And ſeminal vermiculi, inſtead of ceaſing to move and periſhing at a ſmall degree of heat, according to M. De Buffon, ſupport as much as is deſtructive to many other animals; which far from being wonderful is rather congenial to their nature, ſince they live in warm blooded animals, that is, in an atmoſphere in general much warmer than the air, and other fluids where animals are found, and it is fit that they ſhould be able to reſiſt much greater heat.

CHAP. VI.

NEW EXPERIMENTS AND OBSERVATIONS TENDING TO DETERMINE THE NATURE OF SEMINAL VERMICULI.

I FLATTER myſelf that the reader will not be diſpleaſed, if he does not find the ſame order and connection in the reſt of the obſervations, which I have hitherto endeavoured to preſerve. It is neceſſary to conſider what follows as an appendix, which, in my opinion, muſt indiſpenſably be added to prove the animation of the vermiculi

culi with greater certainty; this being one of the chief objects propoſed: and, when once eſtabliſhed, not only will the contradictory opinions concerning the nature of ſpermatic vermiculi, already explained and diſcuſſed, be confuted, but we may alſo anticipate every new hypotheſis that can be ſuggeſted.

One day, during winter, I had abundance of ſemen, taken from the veſſels of a dead body (1); and, wiſhing to preſerve the vermiculi ſome hours alive, put the glaſs containing them in the ſunſhine, on the outſide of a window. The heat of the ſun was 70°, which kept them alive a conſiderable time: but obſerving the vermiculi an hour afterwards, it gave me extreme ſurpriſe to find almoſt the whole motionleſs. I knew not whether this indicated real or apparent death; and, thinking to ſatisfy myſelf by expoſing them to greater heat, I tranſported them near the fire. Experience had taught me how inſtrumental heat is in reſtoring vermiculi to motion. But it was in vain; and although kept here a long time, they manifeſted no ſign of life. It was otherwiſe with thoſe left in the ſhade, and theſe carried to the fire; for there was another portion of ſemen in a watch-glaſs in the ſame apartment. The vermiculi alſo had

(1) When the kind of ſemen is not ſpecified, that of man is always underſtood.

had become motionleſs, but they ſoon reſumed their original vivacity. The ſingularity of the phenomenon made me ſuppoſe it accidental, and I did not think of repeating the experiment during this winter and the following ſpring. But I had afterwards occaſion to obſerve, that the ſun was conſtantly fatal to vermiculi in a few hours; though the intenſity of the heat did not equal the degree which, in the preceding chapter, is ſaid to have been fatal to them. This was aſcertained by means of the ſun in autumn; but the phenomenon, which I at firſt thought accidental, has appeared conſtant and invariable. The ſolar influence, at the ſame time, has no quality noxious to the putredinous animalcula of the ſame ſemen, provided its intenſity does not raiſe the thermometer to 106° or 108°, which alſo contributes to prove the difference between the vermiculi and animalcula.

The novelty of ſuch reſults was a ſufficient incentive for inveſtigating the cauſe. Experiment having ſhewn me that a certain degree of ſolar heat quickly kills vermiculi, though an equal degree in an apartment does no injury, I could not be perſuaded that the ſimple heat of the ſun cauſed their deſtruction, but imagined ſomething entirely different. My firſt idea was, the agitation of the air: I conceived that, by putting the ſemen on the outſide of the window, the vermiculi were more

more affected by the ſenſible influence of this element, and ſooner yielded under it than thoſe within the apartment, where the air was leſs agitated. My ſuppoſition was erroneous; for, putting out two glaſſes with the ſame ſemen, equally expoſed to the air, and only with the difference, that one was in the ſunſhine and the other in the ſhade, thoſe in the ſun always died ſooner than thoſe in the ſhade. Further: a diviſion was put in the ſeminal fluid of the ſame glaſs, ſeparating it into two parts, ſo that one was expoſed to the ſunſhine and the other was not; it uniformly happened, that the vermiculi of this latter portion long ſurvived thoſe of the other.

Attentively contemplating that in the ſolar rays with the naked eye, I ſuſpected another cauſe. The ſemen both greatly diminiſhed, and became denſer; likewiſe, the colour changed. This increaſed denſity might be prejudicial. An eaſy method was employed to aſcertain the fact, which was to prevent the evaporation of the ſemen in the ſun, becauſe the denſity might be occaſioned by evaporation of the more volatile parts; and my purpoſe was attained by hermetically ſealing ſeveral capillary tubes full of ſemen, and then expoſing them to the ſun, along with another portion of ſemen in a watch-glaſs. The thermometer ſtood at 73° in the ſun. Thoſe in the watch-

glaſs

glaſs did not live an hour; but the vermiculi in the capillary tubes retained all their vigour at ſun-ſet, though the experiment was made in the morning; and an hour after mid-day, the heat of the ſun was 104°. On the ſucceeding days, other capillary tubes were put in the ſunſhine as above; the vermiculi, as uſual, were long alive. Therefore theſe repeated facts prove two things; *firſt*, That the ſudden death of vermiculi in ſunſhine is not properly the effect of the ſun, as it would have killed thoſe in the capillary tubes as ſoon as thoſe in the open veſſels; *ſecondly*, We can aſcribe their immediate death only to ſome vicious quality or alteration acquired by the ſemen, when expoſed to the air, and againſt which it is ſecured in a cloſe veſſel. But as it does not ſeem to ariſe from any other cauſe than the inſpiſſating of the ſemen, for putrefaction cannot begin in ſo ſhort a time, we are induced to ſuppoſe this the ſole cauſe of their death, at leaſt an eſſential reaſon.

Theſe facts are illuſtrated by the following experiments: Two glaſs tubes filled with ſemen to a given height, and ſtopped with a ſtopper well fitted, were placed in the ſunſhine: one ſtopper touched the ſemen; the other was an inch above it: the tubes were in an erect poſition, and an equal quantity of ſemen in each. In an hour and a half, the influence of the ſun had occaſioned no evaporation

evaporation in the tube with the ſtopper touching the ſemen; indeed it was impoſſible, as no vacuity was between them; but the ſemen in the other tube had evaporated. The inſide of the glaſs and the end of the ſtopper were covered with a thin pellicle, formed of a very tranſparent fluid, which could be nothing but the more ſubtile parts of the ſemen volatilized by heat. The quantity was diminiſhed, which could not be otherwiſe, and it was a little thicker; neither of theſe circumſtances was remarked in the other tube. Both the fluids were examined with a magnifier: the vermiculi were alive where there was no evaporation; in the other tube all were dead. Thus it is evident, the ſolar heat does not kill the vermiculi; but that their death is occaſioned by ſome noxious quality imparted by it to the ſemen, either conſiſting in that thickneſs, or ſomething elſe derived or generated on the occaſion, which alſo correſponds with the nature of animals that are injured and periſh, if the ambient fluid in which they live, aerial or aqueous, begins to alter and ſpoil.

It ſhould likewiſe be explained how two degrees of heat, equally intenſe, can produce ſuch oppoſite effects: for, the immediate action of ſunſhine kills the vermiculi, while that of a heated apartment does them no injury: but my obſervations have

have not been ſufficiently numerous to ſolve the problem.

We have ſeen that the motion of ſeminal vermiculi continues complete days in confined air, and it ceaſes in a few hours in the open air. I have demonſtrated that the continuance of life in cloſe tubes is the conſequence of their being ſheltered from the influence of the air. From analogy, one would think that this ſhould occur at all ſeaſons. But arguments from induction ſhould not be uſed by philoſophers, ſince they are too often ſupported by deceitful appearances: the preſent caſe may afford a new example. During winter, vermiculi in tubes, ſealed hermetically, become motionleſs in the ſame time as in watch-glaſſes expoſed to the open air: nor is cold equal freezing required to render them motionleſs. In an hour and a half, they were motionleſs in capillary tubes, as well as in the open air, when the thermometer ſtood at 45°. That vermiculi may continue alive in tubes, a certain degree of heat is neceſſary, which my experiments indicate to be 52° or 54°.

When they become motionleſs, it is not always a ſign of death; ſometimes only a ſimple lethargy takes place. When included in tubes, I have, by means of heat, reſtored them to their natural motion from perfect reſt: Motion and reſt have even been produced ſucceſſively, by tranſporting the

the tubes from heat to cold, and *vice verſa*. We muſt remark that repetition of this operation enfeebles the vermiculi ſo much, that at laſt they cannot recover, and periſh for ever.

I wiſhed to learn how long they might remain lethargic without deſtruction, ſo that removing them to a warm ſituation, they would ſtill reſume their original ſtrength and motion, but I have been unable to determine the preciſe limits: it appears to depend in a great meaſure on the degree of cold to which they have been ſubjected. If the cold ſurpaſſes freezing, and we delay ſome hours to remove them to a warm place, then they revive no more, or but a few are re-animated; and theſe are generally weak and ill. If the cold is leſs intenſe, and the thermometer only falls to 41° or 39°, they may remain lethargic fourteen hours, and even longer. Here I do not intend demonſtrating to the reader, that the accidents happening to vermiculi in capillary tubes completely quadrate with thoſe experienced by animals expoſed to cold, and that cold is fatal to them, as to many inſects: this may be underſtood without explanation.

If we ſhould now unite theſe traits with the reſt diſperſed through this work, with the death of vermiculi by the poiſonous exhalations commonly fatal to animalcula, the odour of camphor, the oil of turpentine, the fumes of ſulphur and tobacco,

tobacco, the effluvia of the moſt ardent liquors, and the electric ſpark, as I have proved, we ſhall have an aſſemblage of proofs ſo convincing, ſo deciſive of the real and abſolute animality of ſeminal vermiculi, that I know not what other evidence could be required to prove that atoms, minute as points in matter, are of ſuch a nature as vermiculi ſeem to be.

I muſt juſtify myſelf for the inconſiſtency of what is now related, and the brief account of vermiculi in my firſt treatiſe on infuſions (1). There I ſpoke negligently; I had not then ſtudied them; but truſted to what others had written, and adopted the theory that ſeemed beſt ſupported by facts. I did not heſitate to embrace M. de Buffon's opinion; and, with him, ſuppoſed vermiculi were not real animals, as his ſentiments appeared to be ſupported by obſervations more numerous, better detailed, more connected and convincing, than thoſe of Leeuwenhoeck. At that time this was my opinion; and it would have always continued the ſame, had not the obſervations narrated here convinced me of the contrary; and I flatter myſelf that nobody will reproach me if my preſent ſentiments are different from what they formerly were.

This

(1) Saggio di Oſſervazioni Microſcopiche.

This chapter ſhall be terminated with ſome reflections equally curious and nice, reſpecting our vermiculi. They were communicated to me in a letter from M. Bonnet; and the reader cannot judge better of their import than by having them before him. After informing me of Linnæus' ſingular opinion, that vermiculi are inert corpuſcula floating in the ſemen, he adds, 'I return to the 'ſeminal vermiculi, whoſe exiſtence cannot be 'doubted. Of all animalcula in fluids they are 'thoſe that have excited my curioſity moſt. The 'element in which they live, the place of their 'abode, their figure, motion, ſecret properties, 'all, in a word, ſhould intereſt us in ſo ſingular 'a kind of animated beings. How are they form-'ed there, how propagated, developed, or fed; 'and what is their motion? What becomes of 'them when the liquid they inhabit is returned 'by the veſſels, and mixed with the blood? Why 'do they appear only at the age of puberty, or 'where did they exiſt before this period? Do 'they ſerve no purpoſe but to people that fluid, 'where they are ſo largely diſperſed? How far 'are we from being able to anſwer moſt of theſe 'queſtions? and how probable is it, that future 'ages will be almoſt as ignorant of the whole as 'our own. If, as I have ſaid, in the *Palingeneſie*, "our world has been chiefly made for intelli-

" gence ſuperior to our's, it is that which knows " the hiſtory of ſpermatic vermiculi, and that of " the moſt myſterious productions of the globe," part 12 and 13. You may ſee in Articles 131, 132, 133, 134, 135, of my *Corps Organiſés*, that in my youth I attempted to ſtudy animalcula. Obſerve what is ſaid on this occaſion in Article 135, concerning thoſe of infuſions. " Reſpecting the appearance of animalcula in " ſubſtances that have been boiled or expoſed " to a degree of heat at which we cannot con- " ceive any animal may ſurvive, the difficulty " ought not to ſurpriſe us too much, as it is " founded only on our ignorance of the heat " that certain animals may ſupport. Beſides, " it is not certain that the animalcula were not " in the infuſed ſubſtances. Perhaps they might " inhabit the air confined in the veſſel, and paſs " from it to the infuſion. Perhaps there is a " perpetual circulation of aerial animalcula in " organized bodies, and in bodies organized in " the air.

" No animalcula are better adapted than ſeminal " vermiculi to demonſtrate how well it pleaſes " the Supreme Wiſdom to multiply ſentient be- " ings, and leave no portion of nature void. " Could we have ſuſpected that this precious li- " quid, the reproductive principle of large ani- " mals, is at the ſame time deſtined for the " aliment and pleaſure of an innumerable mul-

" titude

" titude of moſt minute animated beings? Thus " has the Adorable Wiſdom preſided over the " formation of the univerſe, and known to make " the ſame production ſerve for ſuch different " purpoſes.

" The Author of Nature,' ' Have I ſaid, *Contem-* ' *plation, Part.* 5. *Chap.* 17,' " has left nothing " uſeleſs. The pollen conſumed in the genera- " tion of plants is very little compared with the " whole quantity that each flower affords. Wiſ- " dom has therefore created the induſtrious bee, " which uſes the ſuperfluous part of this duſt " with an art that the moſt ſkilful geometers " cannot enough admire. The pollen of the ſta- " mina apparently ſupplies the neceſſities of many " other inſects; and theſe inſects are in ſome re- " ſpect to pollen what vermiculi are to the " ſeminal fluid.

' The origin of certain worms in the human ' body, and that of animals, is a problem yet un- ' ſolved by naturaliſts: ſuch, in particular, is the ' origin of the *Tenia:* in my Diſſertation, I have ' ſpoken at length of this ſingular worm. The ' origin of ſpermatic vermiculi is a problem ſtill ' more profound. However, I ſhould much in- ' cline to preſume, that, like thoſe mentioned in ' my Diſſertation, they originate from without. ' The change of temperature, abode, and nutri- ' ment, may produce firſt in individuals, then in

'ſpecies, very material alterations diſguiſing the 'primitive appearance to our view. A worm 'deſtined to live in the waters, and tranſported 'to our inteſtines, might not periſh, and yet be 'very much diſguiſed, eſpecially if introduced 'when young, or under the form of an egg or of 'ſemen; and if the worm was to propagate, the 'ſubſequent generations would be ſtill more diſ-'guiſed. Let us, therefore, ſuppoſe that the ſe-'mina of certain infuſion animalcula may be in-'troduced into the ſeminal reſervoirs by the cir-'culating ducts: they might be developed and 'live there. No doubt, this new abode, a tem-'perature and aliment ſo different, may greatly 'affect the original form of the animalcula, and 'at length induce changes that may more and 'more remove them from their firſt appearance. 'All mankind had the ſame origin. What va-'rieties, what ſtriking varieties are there in the 'human ſpecies! Let us compare the inhabi-'tants of the frigid zone with thoſe of the tem-'perate regions, and thoſe of this with the inha-'bitants of the torrid zone; and we ſhould ſup-'poſe we ſaw different ſpecies of men (1). The

'ſemina

(1) M. Bonnet extends analogy too far: and himſelf proves the dangers of analogical reaſoning. Whether there are actually different races of men on the earth, which cannot have had the ſame origin, or whether all may

‘ femina of ſome infuſion animalcula are proba-
‘ bly ſo minute, that they may eaſily arrive at the

‘ reſervoirs

may have ſprung from one parent, may well be the ſubject of diſpute. It belongs to the philoſopher to admit nothing but evidence. Prejudice, tradition, and analogy muſt be rejected, elſe he will never attain the way to truth; and facts alone muſt conſtitute the foundation of his knowledge.

This is an important ſubject: it merits mature inveſtigation. From the inquiries of modern naturaliſts, there is undoubtedly great reaſon to believe that there are conſpicuous varieties among the races of men now inhabiting the globe; varieties ſo ſtriking as neither to be affected by climate nor the mode of life. Independent of the ſize of the perſon, the colour of the ſkin and the nature of the hair, on all which climate may in general have a conſiderable effect, there is a difference in the figure of certain bones, and in the length of others that ſeems peculiar to the men. The quantity and proportion of fleſh are variouſly diſtributed over ſome parts of the body: and one of the ſtrongeſt evidences of a diſtinct race is the ſingular conformation of the Egyptian and Abyſſinian women, and thoſe of the interior of Southern Africa.

It is very poſſible that caſual mutilations or natural imperfections may be tranſmitted to poſterity: we have actually witneſſed it on a ſmall ſcale: yet I rather conceive that they would diſappear in ſubſequent generations. But how can we ſuppoſe the numerous and important varie-

ties

'reservoirs of the seminal fluid. Apparently 'they are excluded, but in those seminal fluids 'that have acquired a certain degree of perfec- 'tion, which happens only at the age of pu- 'berty, it would be a most curious experiment 'to try whether infusion animalcula would live 'in some seminal fluids: And in the same man- 'ner, whether the vermiculi would live in infu- 'sions. Above all, the temperature of the place 'and the fluid would need to be regulated. Who 'can say that this experiment, which is certainly 'new, will not succeed? I communicate all my 'ideas to you. My maxim is to despair of no- 'thing in natural history. Why should we say a 'thing is impossible, because we have not seen it 'succeed? This maxim is founded on our pro- 'found ignorance of the secrets of nature, and on 'the deviations she in many cases seems to make 'from her ordinary course. Every where is an 'universal latitude seen, yet I am ignorant of 'the limits. They can be discovered by experi- 'ment alone: and how much may experiments 'of

ties which we behold arise from accidents that individuals were subjected to in the most remote antiquity. Why should it be more difficult to admit variety in the races of men than of any other animated being inhabiting the globe?—T.

‘ of all kinds be combined, multiplied, repeated, ‘ and perfected !’

The difficulty of the queſtions propoſed in this valuable extract is too well defined by its illuſtrious author not to be evident to all who poſſeſs the ſmalleſt portion of philoſophy. It will always afford me a good excuſe for only attempting to anſwer the doubts by diſtant conjectures. The queſtions may be reduced to three. 1. What is the origin of ſeminal vermiculi ? 2. How do they propagate ? 3. What purpoſe do they ſerve ?

As to the firſt, though Bonnet makes no poſitive aſſertion, we perceive his inclination to think ſeminal vermiculi have an external origin. Such has been the opinion of many authors in eſteem ; and ſuch is the opinion of thoſe who ſuppoſe that the worms in the body of man and animals originate from without. Sir Charles Linnæus believes the abode of the *tenia* is in the waters ; there he has found them very ſmall, and even in ſome fiſhes, particularly in tench, which ſeems to favour this opinion (1). But we ſhould be certain of the identity of the ſpecies found in water with that found in the human body, and of this we have not yet had ſufficient proof. We

(1) See the Italian tranſlation of La Contemplation, part 10. ch. 26. Note, at the words, *Molte centenaia di piedi.*

cannot deny, at the same time, that very accurate observations demonstrate, that some worms, if not of the human body, at least of the bodies of particular animals, are produced by insects of the great world. Such are those inhabiting the rectum of the horse, the frontal sinus of sheep, goats, and the larynx of stags, as the eminent and most expert naturalists, Vallisneri and Reaumur, have discovered.

With respect to seminal vermiculi, my observations will not allow me to ascribe them to an external origin; was it so, I must certainly have once perceived it. More than fourteen years have elapsed since I have been occupied with infusions, with studying the waters of marshes, ponds, and ditches, for these are generally full of microscopic animalcula; and, with absolute sincerity, I can affirm, that among the innumerable minute animals contemplated by me, never was one seen resembling the seminal vermiculi of man and quadrupeds (1). I do not deny that, admitting

(1) This had already been remarked by Leeuwenhoeck, as appears from his 301 letter. 'Licet varias, et indole 'diversissimas aquas contemplatus sim, nec istiusmodi ani- 'malcula, (id est spermatica), nec quidquam quod ani- 'malcula ista similitudine aliqua, vel figura referret, in ullis 'unquam aquis observaverim.' Other observers agree with Leeuwenhoeck and myself as much as could be desired. Among 146 genera which Muller has classed, he has seen only

admitting they pass from water to animated bodies, they may possibly undergo some change or alteration, operated perhaps by the causes the Genevese philosopher details, which has been rendered probable by demonstrating, that animals which change their climate and aliment suffer such mutations, 'Ranæ in Ebusum Insulam de-'latæ colores mutant; Oves in regione septen-'trionali albescunt, in meridionali nigrescunt. 'Sic Vulpes, Ursi, Lepores mutato loco, colores 'et quandoque mores mutant.' I likewise acknowledge that the body of vermiculi may change its proportions; that the parts may become larger or smaller according as the new element agrees with their nature; as also happens in plants carried to a foreign soil; but I cannot think they will lose their pristine figure to assume

only one, (which he calls Cercaria), resembling the seminal vermiculi of the ram. But this species, which is the only one that has been observed, and is very seldom found, (in infusione animali raro), would be far from being able to give existence to all the different species of vermiculi.—A.

Muller makes this remark on the *Cercaria Gyrinus*. There are others of the same genus that may resemble seminal vermiculi; and perhaps some of the Trichodæ. But of the many hundred figures in his *Animalcula Infusoria*, a work that no animalculist can want, certainly there are none that may be confounded with the vermiculi: Many, however, have tails unconnected with the motions of the body.—T.

ſume one very different, or that the firſt change will be ſuch as to prevent them being recogniſed, for then the internal ſtructure muſt alſo be changed; and this is as much as to ſay, that after their former organs were wholly or partially changed, they ſhould produce new ones, which would rather be a creation than a change.

The ſeminal vermiculi not only differ from animalcula in ſhape, but I have likewiſe ſhewn that they are of a nature and conſtitution eſſentially different. Such are the differences mentioned in the former chapter and that preceding it, but omitted here to avoid repetition.

M. Bonnet ſuggeſts a moſt ingenious experiment, to try whether ſeminal vermiculi will live in infuſions, and infuſion animalcula in ſeminal fluids. I had already done this in part; but, on tranſmitting the animalcula of putrid into recent ſemen, and the vermiculi of recent ſemen into that which was putrid, the whole animalcula and vermiculi periſhed. Yet, to ſatisfy the curioſity of my illuſtrious friend, inſtead of corrupted ſemen, I uſed vegetable infuſions, and took the precaution to make them of the ſame heat as ſemen in the body of the animal, and that the ſemen where the animalcula were put ſhould have the temperature of the atmoſphere; however their death could never be prevented. There was a little difference: the vermiculi

periſhed

periſhed immediately; the animalcula in a few minutes.

The great difference of aliment to which vermiculi muſt be accuſtomed, on paſſing from without into the ſemen of animals, ſeems to me a ſufficient reaſon why that cannot be their origin. For the ſmall animals of our world periſh when obliged to change their food, as we ſee in caterpillars feeding on ſpecific plants; when the plants are changed they die: ſo that, if we give ſilk worms any other than mulberry leaves they very ſoon ſuffer. That inſects ſhould die, it is not neceſſary the plants ſhould be changed, even a change of the parts that they naturally occupy is ſufficient. Let us conſider thoſe whoſe common abode is on the ſame tree: and how many hundred ſpecies inhabit the pear? Some feed and dwell on the ligneous parts; ſome inſinuate themſelves between the bark and the wood, and never leave them; ſome conceal themſelves by folding ſeveral leaves together, and feed on the moſt delicate parts; others prefer the roots, and, by penetrating, form tumours there; ſome menace the flowers, and others the fruit. Let us change this order; let us tranſport the inſects of the wood into the bark, and reciprocally; thoſe of the leaves to the roots, and ſo on with the reſt; it will not be long before they die. I cannot conceive why the ſame ſhould not happen to

ſpermatic

ſpermatic vermiculi on paſſing from without to the ſemen, ſince their aliment is completely changed. It is not enough to ſay, we have an example in the larvæ found in the horſe, the ſheep, or the ſtag, which live in a place where their exiſtence did not begin; for I may anſwer, they have not paſſed from a large to a little world after living in the former, but are developed in the quadrupeds where they have been depoſited by their parent flies, where they remain until maturity, and feed on the ſubſtance of the animal. If, before acquiring this maturity, their ſituation was fortuitouſly changed, it is moſt likely they would periſh. We ſhould ſee them periſh, or rather not expand, if the flies laid their eggs elſewhere. Whence it follows, that this example confirms the general law.

If we cannot believe that vermiculi come from without, what is their origin? We ſhall anſwer what Valliſneri has ſaid on the origin of the large worms in the human body.—They are produced, nouriſhed, and multiplied in us and other animals:—they paſs from generation to generation with the nutriment the mother affords in the uterus, or with the milk that is imbibed (1). This hypotheſis ſeems to me much more probable than the other. According to M. De Buffon, the ſemen

(1) Valliſneri, T. 2, edition in folio.

men of the female is full of vermiculi perfectly like thofe of the male. I do not doubt that it is truly fo. The fame had been before obferved and defcribed by Signor Bono, a celebrated phyfician and an exellent obferver of fpermatic vermiculi, incapable of altering any fact, as he was unprejudiced in favour of theory. What has been remarked, by thefe two naturalifts, I have fometimes, but rarely, feen in the blood. In my long remarked on the phenomena of circulation, I happened to obferve, in the mefenteric blood of a frog and three newts, I know not how many of the feminal vermiculi peculiar to thefe amphibia. There was no hazard of deception, becaufe there was no room for error. One cannot think, thefe vermiculi were mixed with the blood, from the rupture of fome of the blood veffels of the teſticles or vafa deferentia, fince the frog and two of the newts were females; and the blood veffels, as well as the generative organs of the male, were entire, as I affured myfelf by a careful examination. The vermiculi were actually confined in the veffels, and were moft vivacious. They appeared in the arteries, excepting a fingle time that I faw them in a vein. The artery of a frog tadpole fhewed me fome again: fome were even feen in the blood, ftill warm, of a fucking calf; and feveral among the red globules in the blood of a ram. I could not but recogniſe them,

as

as they had all the characteriſtics of the ſeminal vermiculi, peculiar to the two ſpecies of animals. Theſe obſervations prevented my ſurpriſe, when mixing a drop of ſemen with a drop of blood, ſo that the vermiculi were forced from their proper liquid into the blood, they ſtill lived as before. The ſame has taken place with ſaliva; and it is natural to ſuppoſe it will happen with other animal fluids.

From theſe facts I draw two concluſions; *firſt*, That it is not abſurd to ſuppoſe that the mother may ſerve as a vehicle for the ſeminal vermiculi to tranſmit them to the young; *ſecondly*, That they live in the fluids of the young, particularly the blood, and are, *in a manner*, retained there until puberty. I ſay, *in a manner*, becauſe the rareneſs of ſeminal vermiculi in the blood ſufficiently proves it is not a fluid that agrees too well with them; perhaps, becauſe the aliment found there is not very well adapted to their nature; and although the ſemen is derived from the blood, they are two very different fluids. To this it may be oppoſed, that the vermiculi found in the blood of the male are re-abſorbed by the veſſels, and mixed with the maſs of the blood. The objection would be well founded if applied only to adult males; but it is inſufficient where they are not ſo, as tadpoles and ſucking calves. The organs of generation are not unfolded in the

the former; and thoſe of the latter are not inhabited by vermiculi.

Beſides the mother, the father may be an inſtrumental cauſe in the propagation of vermiculi: which may take place with almoſt every ſpecies of animal. This method is at leaſt more direct, or perhaps more natural than the other: I ſpeak of the act of fecundation, which may convey the germs of the vermiculi to the embryo by the immediate vehicle of the ſemen. That the eggs of females may be impregnated, they muſt be bedewed by the ſpermatic fluid of the male, which muſt act on the included embryo: it ſhould act not only externally, but alſo internally: for we know that it regulates the parts of the fœtus (1). Therefore it is neceſſary that the fœtus ſhould be penetrated by it; and in this way may the vermicular germs be eaſily introduced. Theſe expand or lay the foundation of a little colony of vermiculi, which take poſſeſſion of the ſeminal fluid, and give birth to numerous inhabitants.

The ſecond queſtion relates to the mode in which vermiculi propagate. In the number of experiments, almoſt infinite, that I have made, my attention has always been directed to this intereſting point. After ſeeing a prodigious multitude of infuſion animalcula multiply by diviſion of the body,

(1) Bonnet, Pref. a la Contemplation.

body, I inveſtigated whether ſeminal vermiculi propagated in the ſame manner, but there has been no indication of this. It is true, when they paſs from the animal's body, or when the vermiculi begin to be in a morbid ſtate, they are leſs fit for dividing than when in their natural ſtate in the ſeminal reſervoirs when vigorous and full of life. I do not deny that this is poſſible; but admitting it was ſo, it ſeems morally impoſſible that, among ſo many millions of vermiculi, which I have at different times obſerved: among ſo many ſpecies, there was not one in a ſtate to divide, or which did divide, as is ſeen with the animalcula of infuſions. Neither have I obſerved that vermiculi propagate by buds or ſhoots, like polypi. Therefore, abiding by the different modes of propagation hitherto known, it would appear a proper concluſion, that ſeminal vermiculi multiply by means of a fœtus, or of eggs; but I muſt admit, that neither the one nor the other has been ſeen by me.

I come to the laſt queſtion. What is the uſe of the vermiculi? Leeuwenhoeck's opinion is well known: He thinks they are the immediate authors of generation; ſo that thoſe of man will be ſo many homunculi; thoſe of the bull, ſo many vituli; thoſe of the horſe, ſo many foals; and thus of the reſt. We cannot deny the idea is very ingenious: it is unfortunate that it is not

real.

real. I ſhould deviate from my plan, by attempting to refute it. Authors, celebrated and known to natural philoſophers, have done ſo with ſucceſs. But I cannot reſiſt ſaying a word on the beautiful diſcovery of Haller, which is completely deciſive. He proves, by facts ſo convincing that it is impoſſible to withhold our aſſent, that the fœtus belongs to the female; that entire, it pre-exiſts fecundation (1). It is evident, therefore, that the vermiculi afforded by the male cannot be fœtuſes. The facts eſtabliſhing this diſcovery are explained in his excellent treatiſe on the chicken.

Though we refuſe the vermiculi this noble purpoſe, we do not fail to aſcribe others to them. Some phyſiologiſts have ſuppoſed them the cauſe of venereal pleaſures; others, that they preſerve the fluidity of the ſemen; and ſome, that they effect certain unknown purpoſes. All have endeavoured to divine their uſe.—There exiſts in us, and in other animals, this wonderful multitude of living beings, and many larger vermes, of which

(1) The author diſcovered that the tadpoles of frogs, toads, and newts, pre-exiſted fecundation; that they exiſted in the egg produced by the mother, before it was moiſtened by the ſemen of the male, *Diſſertations on Animals and Vegetables*. He alſo diſcovered, that, in the egg of the torpedo, a fœtus exiſts previous to fecundation, *Lettera Sopra Diverſe Produzione Marine*.—T.

the eminent Redi has given a hiſtory in a work on the ſubject (1). Beginning with man, and deſcending to animals, not excepting the ſmalleſt, each in like manner has external inſects which it feeds, as the ſame naturaliſt proves in another treatiſe (2). But why do all theſe races of animals inhabit the internal and external parts of others? Why were they created? This, I think, is beyond the ſphere of human knowledge; and it will not diſpleaſe, if here I ſhould be ſilent. And I hope I ſhall be pardoned for only gueſſing at the other two; their intricacy has prevented me from doing more.

The intent of this tract was to examine, with all poſſible attention and accuracy, the properties and nature of the myſterious inhabitants of animal ſeminal fluids; and I have taken the liberty to diſcuſs and refute the opinion of others, learned in the ſubject, becauſe their celebrity and diviſion had created doubt. The intelligent, judicious, and impartial reader is left to conſider whether I have ſucceeded, and whether I have diſſipated, at leaſt diminiſhed, the obſcurity that veiled them from the truth.

(1) Degli animali viventi negli animali viventi.

(2) Eſperienze intorno agli inſetti.

OBSER-

OBSERVATIONS AND EXPERIMENTS

ON

ANIMALS AND VEGETABLES CONFINED IN STAGNANT AIR.

INTRODUCTION.

If privation of air is a powerful method of preventing the production of animated beings, and of deſtroying life in thoſe already in exiſtence, the preſence of air itſelf, when its circulation is impeded, is conſidered equally noxious. A general rule has been eſtabliſhed, that all animals and vegetables forced to reſpire the air of cloſe veſſels periſh irrecoverably. It is thought equally certain, that ſeeds do not germinate, and eggs are not excluded in this ſituation. The great Boerhaave ſpeaks thus of all. "Ovula quo-"rumcumque inſectorum in vitris accurate clauſis "non producunt, licet tepore fota, fœtus; ſemina "plantarum vite macerata, optimæ commiſſa ter-

" ræ, atque requiſito excitata calore, non tamen " creſcunt, neque dant vitæ ulla ſigna actuo- " ſæ (1)."

Such is the received opinion of all philoſophers and naturaliſts, which for many years I confidently adopted, and believed certain with reſpect to the animal and vegetable kingdoms. But my experiments on infuſions inſpired me with a juſt diſtruſt. I had diſcovered that the animalcula of infuſions were produced, and lived, in veſſels hermetically ſealed; and that the ſeeds uſed for the infuſions germinated. Theſe facts did not correſpond with the general belief; and excited a lively deſire of inſtituting a number of experiments to inveſtigate the limits and conditions of the phyſical maxim, That air which is confined and cannot be renewed is noxious to beings in life, whether animals or vegetables. With this deſign, I reſolved to repeat and to diverſify my experiments on infuſion animalcula and vegetable ſeeds growing in cloſe veſſels; and alſo to make experiments on ſome animals, whoſe great analogy to infuſion animalcula would induce us to ſuppoſe that the noxious influence of ſtagnant air could not ſo eaſily affect them as it would affect animals ranking higher in the ſcale of beings. My experiments were therefore made on the eggs of many terreſtrial and aquatic inſects,

(1) Elem. Chem.

fects; and nature has afforded thoſe illuſtrations which former authors have ſought in vain.

From effects I aſcended to cauſes, and inveſtigated why confined air could, in certain ſituations, be injurious to animated beings. Thus, paſſing from one reſearch to another, the work inſenſibly increaſed in my hands, and became much more conſiderable than I expected. Since I now preſume to publiſh it, that the tedium of the reader may be leſſened, it is divided into three ſections or chapters. The principal objects of the two laſt are reſearches on the cauſe of animals dying in confined air; and the firſt comprehends a view of thoſe beings that are ſubject to its influence only in certain circumſtances.

CHAP. I.

INFUSIONS, VEGETABLE SEEDS, ANIMALS EGGS, AND ANIMALS THEMSELVES, SUBJECTED TO STAGNANT AIR.

I PROVIDED a certain number of veſſels: In each I put an infuſion of vegetable ſeeds, and then ſealed them hermetically. They were made expreſsly for the purpoſe, large, and each might

contain fourteen or fifteen pounds of water (1). It was not neceſſary to break the veſſels for examining the infuſions, but ſufficient to cauſe the included liquid flow to a dry part of the veſſel; and, after it had flowed back, to obſerve the part that had been wet, with a powerful magnifier. The glaſs was ſo tranſparent, that animalcula, if any were produced, could be ſeen ſwimming in the thin pellicle adhering to the internal ſurface of the vaſe.

My experiments were made towards the end of ſpring; and it was not long before animalcula began to inhabit the veſſels, and inhabit the whole. The periods of their increaſe, diminution, and deſtruction, were the ſame as in the open air.

On repeating the ſame experiment often with different ſeeds, all afforded animalcula. One difference was to be remarked. The number was never ſo prodigiouſly great which I have obſerved before. One mode by which theſe animals propagate is, the natural diviſion of the body; and this diviſion alſo ſucceeds in confined air. Applying the magnifier to the ſide of the veſſel, I ſometimes ſaw them dividing through the middle; ſo that the parts were connected by a ſhort filament; others appeared like two minute elongated ſpheres.

(1) Twelve ounces in the pound.

ſpheres, touching in ſeveral points; others exhibited a contraction, or rudiments of diviſion hardly begun, on the outſide of the body.

The duration of life, and the multiplication obſerved in infuſion animalcula, were likewiſe ſeen in the anguillae of vinegar confined in cloſe veſſels. From the beginning of April to the end of November, they were viſible, and continually became more numerous. It is true, as winter advanced, the eels periſhed; but the ſame happened to thoſe of vinegar in the open air, which was occaſioned by the increaſed cold. We know that vinegar is without eels during winter.

While I made theſe experiments, the water of ſeveral ditches was full of worms, inſects, and the tadpoles of frogs. On them were repeated the ſame experiments as had been made on the animalcula of infuſions and the eels of vinegar. I began with the larvæ of muſkitoes. Many were confined in veſſels with ditch water, that they might find aliment among the quantity of heterogeneous matter which it was full of. The larvæ ſuffered nothing from confinement: all changed to nymphs, which in time produced the flies.

Tadpoles were alſo confined in veſſels, along with a ſufficient quantity of water, and marſh lentil for food. For twenty-four days, their ſize encreaſed conſiderably; and they died leſs from ſtagnation of the air, it is probable, than be-

cauſe the lentil was completely conſumed. The tadpoles were young: I repeated the experiment on grown tadpoles, whoſe limbs had already begun to appear; chuſing this period to learn whether they underwent their metamorphoſis in ſtagnant air. Several did undergo it, and diveſted themſelves of the tadpole form to aſſume that of the frog; but others periſhed before attaining their new ſtate.

Animals inhabiting the waters are not under the ſame neceſſity of continually reſpiring air, as thoſe deſtined by nature to live amidſt the air itſelf: therefore it was fit to make ſome experiments on the latter. And having made ſeveral on tadpoles, that is, on a ſpecies of animals changing their ſtate, I was deſirous to repeat them on other animals of the ſame nature. Caterpillars immediately occurred; and my firſt experiments were on ſilk worms. Thoſe that were taken would in a few days have begun to ſpin their web. Leaving them on a mulberry branch, the ligneous extremity of which was immerſed in water, that the verdure of the leaves might be ſome time preſerved and ſerve for food, I ſealed them hermetically in a veſſel. Above a third periſhed; but the reſt, being eleven in number, proceeded as uſual; they worked the accuſtomed web, fixed to the ſide of the veſſel, and there ſhut themſelves up. Butterflies came

from

from nine webs: from two, there came none. On examining the ſterile webs, I found the caterpillar transformed to a chryſalis; but the butterfly did not come out, as it died while in this ſtate. The webs of the eleven caterpillars in this cloſe veſſel were of good ſilk; and their only difference from others was in not being ſo hard and elaſtic as cocons generally are.

Other caterpillars, eſpecially thoſe of the elm and oak, underwent changes ſimilar to the ſilkworms in cloſe veſſels. The ſame method was adopted of confining them in veſſels where the lower extremities of branches had been immerſed in water.

The metamorphoſes of the larvæ of large fleſh flies were more diſtinctly ſeen. I put a piece of fleſh nearly putrid at the bottom of one of the veſſels: it ſerved them for food nine days, that is, all the time they were worms, and until they became nymphs. We know, that when their change approaches, they abandon the putrid fleſh, and, ſeeking a dry ſituation, moſt commonly conceal themſelves under an arid duſty earth. Thoſe confined likewiſe abandoned the fleſh, traverſed the veſſel, and were in conſtant motion more than half a day. Their anxiety to eſcape was evident; but, unable to effect it, they retired to the ſides at the extremity of the neck of the veſſel, which was almoſt parallel with the floor, and

became

became perfectly tranquil. There they inſenſibly contracted; their ſhape and colour diſappeared; they aſſumed a light cheſnut ſhade; and exhibited every ſign of being changed to nymphs. In this ſtate, they remained fourteen days; then, the ſhell burſting, flies completely reſembling the parent fly eſcaped. The winged inſects lived ſeveral days in their priſon, and died apparently for want of food.

A few words may be ſaid concerning the ſeeds uſed for infuſions. They developed like the animalcula; all germinated well; and in a few days, the budding and branching of the leaves filled the capacity of my veſſels. I ſhould not omit obſerving, that the whole of theſe vegetations ſhewed ſymptoms of diſeaſe, whether from decaying before fructification or from their yellowiſh colour. Suſpecting that this diſeaſe was not ſo much the effect of privation of circulating air as of the beneficent influence of the ſun, and the moiſture that the roots muſt ſupply, which could not be obtained from the ſmall quantity of water, I endeavoured to aſcertain the real cauſe. The ſame quantity of ſeeds was put into veſſels hermetically ſealed, and, inſtead of water, I ſubſtituted a portion of well moiſtened earth. The plants ſoon ſprung up; and, being expoſed in the ſun beams ſome hours of the day, in a ſhort time, reached the ſummit of the veſſel without becoming

coming yellow till after a conſiderable interval. The ſeeds were peaſe, maize, red wheat, and rye. I planted two ſtalks of rye under the neck of the veſſel, where there was a ſufficient ſpace for their elevation: the neck being very long, they exhibited unequivocal marks of incipient fructification by the ear ſhooting out of the calyx; and it would have made the greateſt progreſs but for the approach of winter.

To complete what has been ſaid of ſeeds confined in veſſels, it may be remarked, that I have in this way obſerved more than twenty ſpecies at different times without finding one that did not germinate. A precaution eſſential to their production muſt be attended to: whenever they ſpring in water, which takes place in cloſe as well as in open veſſels, the infuſed ſeeds muſt be partially above the water, otherwiſe they periſh: which was adverted to before me by the celebrated naturaliſt M. Duhamel.

If vegetable ſeeds germinate without exception in confined air, what are we to think of animal ſemina, or the eggs of inſects, which, according to Boerhaave, and the general opinion of philoſophers, ſhould remain ſterile, even when the operation of circumſtances the moſt favourable to their production concurs? Here I thought it better to conſult nature than to truſt to the ſentiments of others. I therefore made experiments on many eggs: on thoſe of beetles, flies, fleſh flies,

flies, nocturnal and diurnal butterflies, worms, and others, and ſcrupulouſly obſerved what happened to each kind. I foreſee the reader's anxiety to learn the reſult of theſe experiments; and in two words his curioſity may be ſatisfied, by learning that the whole different ſpecies were excluded equally in confined as in open air. Boerhaave, adopting the received maxim concerning the ſterility of eggs in confined air, thus expreſſes himſelf in his Prælectiones Academicæ (1). 'Ova bombycis in aere calido excluduntur, ſi libere admittatur. Eadem in phiala clauſa nunquam producunt ſuum animal.' Now, the truth is, theſe hatch very well in cloſe veſſels, as I have been convinced by every experiment I made. From all that has been ſaid, we muſt conclude, that the air of cloſe veſſels is not an impediment to the production of plants or animals; but plants, without any exception known to me, germinate there, and animals grow and propagate their ſpecies. Thoſe which undergo metamorphoſes experience the changes in cloſe equally as in open veſſels.

Why do we generally believe that ſtagnant air obſtructs the production of animals and vegetables? Analogy is ſurely the cauſe of this remarkable error. It was obſerved, that animals and vegetables ſubjected to experiment ſoon periſhed

(1) Tom. 2.

periſhed in cloſe veſſels; that ſeeds and eggs confined were ſterile; and a general concluſion was made, that ſtagnant air was decidedly noxious to both kinds of living beings.

In the beginning of this chapter, it is ſaid, the veſſels employed were very large; that each might contain fourteen or fifteen pounds of water; with them the reſults enumerated were obtained. But the conſequences are very different on uſing veſſels ſucceſſively ſmaller: In proportion as their capacity decreaſes, the eggs or ſeeds do not germinate, or they periſh when ſcarcely evolved, and animals of every ſpecies in a ſhort time die. All the naturaliſts, who have had reſults different from mine, certainly have uſed veſſels too ſmall, otherwiſe the ſame would have happened as to myſelf. However, I do not deny that their error, reſpecting animals, might ariſe from the nature of the animals on which their experiments were made, that they muſt periſh whatever the capacity of the veſſels may be, as, for inſtance, warm blooded animals. But to underſtand this better, and enforce conviction, let us deſcend to particulars.

Animalcula of infuſions originate, exiſt, and propagate in the large veſſels; which alſo ſucceeds in veſſels one third of the ſize, only we then begin to perceive the diſadvantages of ſtagnant air. When the capacity is ſuch as to contain

three

three pounds and a half of water, the number is leſs; they multiply leſs, and die ſooner. On diminiſhing the veſſels, the larger animalcula are not ſeen, and neither large nor ſmall, if the internal capacity does not exceed ſeven or eight inches.

The nymphs of gnats ſeem to ſupport this ſituation better than infuſion animalcula. In five inches of air, many changed to the winged ſtate. As the quantity leſſens, they periſh proportionally ſooner, and without changing.

The eels of white vinegar are particularly remarkable. They live and multiply prodigiouſly, where the volume of air does not exceed three inches; and die in ſeveral days only, when confined in a tube where the vacuum is leſs than an inch. I ſpeak of white vinegar, for the effects are very different with red. In my experiments, the eels of the latter did not live five days in a veſſel where the vacuum was eleven inches. This did not happen ſo much becauſe the vinegar underwent an alteration, as that the eels of red vinegar rather are of a different nature from thoſe of white; which I believe to be more probable, from the difference of figure I remarked in each ſpecies.

Tadpoles periſhed in ſome days in nine inches of air; and in a few hours, if the vacuum was but three inches.

Caterpillars,

Caterpillars, and the larvæ of fleſh-flies, confined in eleven inches of air, died before becoming chryſalids. The larvæ in particular, ſoon after confinement, deſerted the putrid fleſh put in along with them for food, and tumultuouſly traverſed the veſſel, diſregarding the fleſh. They became motionleſs, and died after various periods, longer or ſhorter: if the veſſel was larger, they lived longer; if ſmaller, they died ſooner. Theſe larvæ, when changed to nymphs, ſuffered leſs from the ſmall quantity of air. In a veſſel where the larvæ had died, I confined the ſame number of their nymphs. The flies of ſome came out; but it muſt be remarked, that the body and wings were diſtorted: they ſeemed to have been produced, as one may ſay, againſt the will of nature. This never happened to the chryſalids of butterflies, though the vacuum was very ſmall.

What has been hitherto ſaid will apply to ſeeds and eggs. I omit telling the reader my trouble in finding the ſucceſſive capacities of the veſſels, where eggs and ſeeds ceaſed to germinate; but adopting the general reſult, when the capacity of the veſſels was but three or four inches, neither eggs nor ſeeds have developed.

Thus it is neceſſary to conclude, that the production of vegetables, and of ſome animals, takes place nearly as well in confined as in open air, provided the quantity in the veſſels is conſiderable;

able; on the contrary, when it is not, that it becomes fatal to both. The precise quantity which may be noxious can only be determined by the nature, constitution, and quality of the animals and vegetables confined.

By experiments in different seasons, I discovered that the death of animals is not only accelerated by diminishing the size of the vessels, as I have shewn, but also by the increase of heat. This is particularly seen in animals which are easily procured at any time of the year, and live long without food; such as, newts, leeches, land and water serpents, vipers, and some species of fishes. I was as careful as possible to take the individuals, for my experiments, of an equal size and equally vigorous, so that the comparisons might be the more just. While engaged with very different matters, I discovered this new fact in the following manner. On the fifth of April, along with other things, I prepared three jars; the first might contain six pounds of water; the second, four; and the third, two. Four newts were confined in each. My experiments were directed to investigate whether the animals died sooner as the volume of air diminished; and I found it to be the case. The four newts in the smallest vessel perished in forty-one hours; in the intermediate vessel, in two days; and in the largest, in seven days.

On

On the ſame day, a ſimilar experiment was made with leeches. I confined four in each veſſel. They lived long in compariſon to the newts. In the ſmalleſt veſſel, they died in three days; in the next, in nine; and in the largeſt, in thirty-two days.

The experiment was repeated, 12 May, on both kinds of animals. They died much ſooner. The newts, in the ſmalleſt veſſel, periſhed in twenty-ſeven hours; in the next, in three days; and in the largeſt, in four days. The leeches, in the ſmalleſt veſſel, died in two days; in the next, in five; and in the largeſt, in nine. The more immediate death of the newts and leeches in May, I ſuſpected was occaſioned by the increaſed heat of the ſeaſon; for, during the month of April, in the greateſt heat, the thermometer roſe to 57°, while, in May, it ſtood at 70°. My ſuſpicions were realiſed by the death of the animals being accelerated in June and July. The thermometer ſtanding at 81°, during the latter, four newts in the largeſt veſſel died in twenty-three hours, and the leeches in thirty-five.

What has been ſaid of leeches and newts will apply to ſnakes, vipers, and fiſhes; becauſe the reſult of experiments made on them correſponded with the former. The death of the whole was not only accelerated in proportion to the ſmall quantity of air they were forced to reſpire,

but alſo in proportion to the increaſe of heat. Twice I obſerved the reverſe, which we muſt conſider as ariſing from ſome accidental circumſtance.

I delayed until winter to make the experiment inverſely; that is, to learn whether the death of animals was retarded in proportion to the increaſe of cold. The experiment ſucceeded with vipers and newts, which were the animals I then had at command. The newts lived twenty-two days in the ſmalleſt veſſel; in the middle-ſized, thirty-four; and in the largeſt, two months. Vipers ſurvived ſtill longer. The veſſels were in a ſituation where the thermometer ſtood at 48°.

Life was protracted in a greater degree of cold. When the veſſels were kept under ſnow, or, which is the ſame, at freezing, the animals did not periſh in three months. On taking them from the ſnow, and expoſing them ſome days to the atmoſphere, during ſpring, both kinds died.

Such are the principal reſults which I have been able to collect from the experiments related in this chapter; reſults moſt uſeful in themſelves, becauſe they elucidate the ſubject: but they leave us to ſuppoſe the cauſe, or rather induce us to ſeek for it; which the reader will endeavour to diſcover, if he is a philoſopher. Why is the death of animals accelerated in ſmall veſſels, and retarded in large? Why is it accelerated more by-

by heat than cold? Whence arifes the difference of time in the death of animals? Why may one quantity of air be noxious to one fpecies of animals and indifferent to another? The folution of thefe problems depends on our knowledge of the caufe of death in ftagnant air. This ancient and moft famous queftion has always divided celebrated modern philofophers. It is important to enter on the difcuffion of it; and I fhall examine what has been already written on the fubject, and adopt that opinion which to me feems moft confiftent with facts, that is, with truth. Since the eggs of animals, and feeds of plants, in a fmall quantity of air, remain fterile, I fhall not fail to add a fhort fentence or two on the caufe of their fterility.

CHAP. II.

TWO PRINCIPAL OPINIONS ON THE CAUSE OF ANIMALS DYING IN STAGNANT AIR.—WHETHER IT LIES IN THE DIMINISHED ELASTICITY OF THE AIR.

Two phenomena have been remarked by thofe who have killed animals in clofe veffels; *firft*, That a quantity of vapour, exhaled from the ani-

mal, accumulates on the sides of the vessels; *secondly*, That the air has lost a certain degree of its elasticity. These phenomena have produced different opinions. One ascribes the death of animals to these exhalations, which, being confined in the vessels, are respired by the animals, and thus become fatal. Another opinion maintains, that the exhalations cannot be mortal; but the elasticity of the air being diminished by them, or a portion of the air being destroyed by respiration, becomes fatal.

The experiment of Pistorini of Bologna, instituted to appreciate the force of both opinions, is specious. Supposing they are equally just, he argues, it should necessarily ensue that two animals, confined in the same vessel, die sooner than one alone, provided the vessel is the same, and the animals of the same size and species. We must, therefore, recur to the exhalations from the animal, or to diminished elasticity of the air occasioned by the effluvia or the respiration; and as it is always certain, that doubling the number of animals, the exhalations and respirations are doubled, the diminution of elasticity should consequently be doubled. But Pistorini found it otherwise. Two animals died as soon as one, though he used the same vessel, and animals of the same size and species (1).

It

(1) Act. Bon. tom. 2, part 1.

It was to be ſuppoſed, that the ſingular conſequences of the experiment induced others to repeat it; among whom was the celebrated Profeſſor Veratti. His experiments were made on birds and frogs. A pigeon lived three hours and three quarters: two pigeons, confined together, lived only half the time. Three ſwallows died in little more than half an hour; two ſwallows in leſs than an hour; and one lived almoſt two hours. He obſerved nearly the ſame with ſparrows and quails; three died ſooner than two, and two ſooner than one. But with frogs it was altogether different: in eight days, four died as ſoon as two, and one alone lived no longer than three, though the veſſel was always the ſame. So that the experiments on birds were very different from thoſe of Piſtorini; and the experiments on frogs agreed with his. And here is ſeen the different nature of animals in this mode of death. Thus far Sig. Veratti (1).

Sig. Cigna, a profeſſor equally celebrated, has alſo engaged in an examination of theſe diſcrepancies, which he thinks to have removed by the accuracy of his experiments. The reſult is this: Where the frogs confined are deprived of water, as it would appear thoſe of Veratti were, there it is true that the plurality never or ſeldom accelerates death. When confined along with water,

(1) T. cit.

which is their natural aliment, it is almoſt certain that the acceleration of death is in proportion to their numbers (1).

The experiments related in the preceding chapter induced me likewiſe to engage in this inquiry. If it was true that ſeveral animals of a given ſpecies confined in the ſame veſſel died as ſoon as where there was only one, and if the phenomena was not accidental but conſtant, it muſt be regulated, as I ſaid before, in a manner proportioned to the ſmaller quantity of air confined along with the animals. But we may eaſily ſee, when the veſſels are equal, that the ſmalleſt quantity muſt always be where there are moſt animals. As frogs had diſturbed the order eſtabliſhed by this law, according to Veratti, and as I could not repeat the experiments of Piſtorini, becauſe he has not ſpecified the animals he employed, my experiments were on frogs alſo: ſome in veſſels with water, and ſome in veſſels without it, thus to approach the method followed by theſe authors. In three veſſels, which would each contain five pounds of water, I hermetically ſealed up frogs, that is, two in one, four in another, and eight in a third. In this laſt, the eight frogs periſhed in twenty-ſix hours; in the ſecond, the four frogs died in one day; and in the other, the two periſhed in two days.

A ſimilar experiment was at the ſame time made with three veſſels more, as large as the firſt,

(1) Miſcell. Taur. Tom. 2.

firſt, and the frogs diſtributed in the ſame manner: ſo that there was no difference between this and the preceding experiment, except that there was no water in the veſſels; and here there were four ounces in each. In two days, none of the frogs were alive in the veſſel with eight; in that with four, they lived three days and a half, and five days where two were. During theſe experiments, the thermometer ſtood between 63° and 70°.

Both experiments were repeated, the circumſtances in every reſpect the ſame, except that the heat of the weather was greater; and then the thermometer aſcended to 77°. The frogs in the firſt veſſel died in twenty hours; in the ſecond, in nineteen; and the two frogs in the third, in about two days. As to the ſecond experiment with veſſels containing about four ounces of water; the eight frogs died in thirty-two hours, the four in two days, and the two in the third veſſel, in three days and a half.

The experiments were repeated ſeveral times; but, to avoid the tedium of dry details, I ſhall not circumſtantially deſcribe them, and will only ſpeak of the reſults obtained from veſſels without water. Sometimes I obſerved the irregularities which have already been remarked. It ſometimes happened, that more frogs periſhed in the ſame time, and ſometimes later than a ſmaller

number. But when the frogs were in water, they conſtantly periſhed ſooner as their number was greater: eight died firſt, then four, and laſtly two. It happened only once that all the eight were alive, when one in the veſſel with four was dead.

From all theſe facts, added to thoſe related by Sig. Cigna, I was ſatisfied that frogs corroborate the general rule, that all animals, without exception, periſh in confined air ſooner according as their number is increaſed. However, we ſee diſcrepancies with frogs included in veſſels without water: but I know not whether they ſhould really be conſidered ſuch, becauſe privation of water is injurious to theſe animals, which Sig. Cigna remarks. Frogs die in a ſhort time in open veſſels wanting water: therefore it is abſolutely neceſſary to proſcribe this as a cauſe diſturbing our experiments.

After finding the reaſon why differences appeared with frogs, perhaps it would not be difficult to find it in Piſtorini's animals, had he mentioned what they were, and the manner of conducting his experiments. With reſpect to Sig. Veratti, we know that he generally uſed birds. But he has found, that they, as I have myſelf done, and ſhall ſoon obſerve, agree well with the rule laid down. Thus there is room to ſuſpect, that Piſtorini's experiment has met with ſome accident,

cident, without knowing in what that accident consisted, which rendered his results different from those of others. It might be occasioned by the birds themselves; perhaps that which he confined alone was less vigorous than those he confined together; whence all died in the same time. Perhaps in his experiment with the two birds, all communication with the external air had not been prevented, which might easily happen if the top of the vessel was covered only with leather, or any substance of a similar nature; or if, on inverting the mouth of the vessel, it had not been well fixed to the plane of position with mastic, glue, or the like. It is very possible that some invisible hole might remain, or some opening, by which the air might get admission. To obviate all suspicion of foreign air, it is necessary to seal the vessel hermetically, or immerse the mouth deep in water, as will be more clearly explained. In the preceding chapter, we have seen how much the heat of the weather accelerates the death of the animals confined. May we not suspect that Pistorini made the experiment on a single bird in very warm weather, and on two in very cold: and that the death of the two birds was by this means retarded by the cold of the atmosphere, which would protract life as long as the single bird survived?

Let

Let us leave this irregularity: but before resuming my principal object, it will be proper to glance at a doubt that occurred on seeing animals die sooner in a close than an open situation, when the number was encreased; and this was, whether more immediate death arose from diminution of the volume of air only, or if the number would occasion it in another manner, and thus become a new cause. To ascertain which was the case, I selected three equal vessels, and a certain number of the largest frogs, as nearly of the same size as possible. Two I confined in one vessel, along with a pound of water, and one put alone into each of the other two vessels, adding a quantity of water equal to the bulk of a frog: for discovering which, I immersed it in a vessel, and observed the quantity overflowing. The quantities of air in the three vessels were equalized by this method, though in one were three frogs, and in each of the others only one. If the greater number of animals accelerated death absolutely from diminishing the volume of air, as the quantity was equal in all the three vessels, the four frogs should die in the same time nearly. If numbers influenced the acceleration of death, the two in one vessel should die first. I have said nearly, for it would be very remarkable if the animals died exactly in the same time. The two frogs in the first vessel lived

ed two days; that in the ſecond died in three days and ſeventeen hours; and that in the third, died in three days and a half. This demonſtrates the influence of number in accelerating death, and other experiments confirm it beyond contradiction. I have repeated the ſame experiment five times, and obtained the ſame reſults. The two frogs in the firſt veſſel always died ſooner than thoſe in the ſecond and third, and the difference of time was very perceptible; it has uniformly been a day, ſometimes a day and a half, and ſometimes longer.

Inſtead of two frogs, I put three in the firſt veſſel, and only one in the reſt; but equalizing the volume of air, by adding a quantity of water equal to two frogs in bulk. The three frogs not only died ſooner than thoſe in the other two veſſels, but the difference was ſtill more conſpicuous than before: it was two days and one hour with the frogs in the ſecond; and two days and ſeven hours with the frogs in the third veſſel. By increaſing the number of frogs in the firſt veſſel, their death was conſtantly accelerated with reſpect to thoſe in the ſecond and third, though precautions were taken to equalize the volume of air by addition of water equal to the bulk of the frogs in the firſt veſſel.

I extended my experiments, and changed the ſubjects to ſeveral ſmall quadrupeds and birds.

But

But notwithſtanding the equal quantity of air in the veſſels, I conſtantly found that they died ſooner as they were more numerous; and that the acceleration of death was always in proportion to the encreaſe of number. Thus, it is evident, that the death of animals, in cloſe veſſels, invariably happen ſooner as the animals are more numerous.

By what phyſical agent, by what means are they deſtroyed? Is it by their breath, or perhaps by injuring the quality of the air which they reſpire? Let us carefully examine theſe two hypotheſes, beginning with that which is founded on an alteration of the ſtate of the air. That it does loſe part of its elaſticity is evident from barometers put in the veſſels where animals are confined. Stair obſerved that his fell an inch in one where a rat died. The deſcent varies. In Veratti's experiments, it ſometimes fell eight lines, ſometimes nine, twelve, or more, according to the nature of the animals. The experiments of Mayow, Boyle, Hales, and others, agree in eſtabliſhing that a portion of the air is deſtroyed by animals confined in a cloſe veſſel. We cannot conclude from them, that the death of animals in cloſe veſſels is owing to the diminiſhed elaſticity of the air; at leaſt there are no experiments poſitively proving the fact. It is firſt neceſſary to enquire, whether an alteration of the air always takes

takes place, when animals die in close vessels. Secondly, whether the degree of alteration is sufficient to kill the animals; for we know that every degree of diminished elasticity is not fatal to them.

Here Sig. Cigna has laboured in a manner that merits commendation. I shall afterwards employ some of his ideas. I have made a course of experiments, with the same view, which shall be abbreviated after relating the method adopted. Several air-pump glass receivers were inverted in a vessel of water. They opened and shut above by means of a metal stop-cock. The receiver being left open when immersed, a free passage was left for the internal air to escape above, in proportion as it was compressed by the rising water; thus the remanent portion preserved the natural degree of density as the external air; which was absolutely necessary for the accuracy of the experiment. This done, I closed the vessel, and, to make it more secure, passed several folds of leather round the stop-cock, to cut off all communication with the external air. I was certain that it could not insinuate itself, for the same receivers were used as in my pneumatic experiments. The animals being put in the receivers, the diminished elasticity could be seen by the ascent of the water within. If the animals were aquatic, or amphibious, I let them remain

in

in water. If terreſtrial, they were put in a veſſel which was ſuſpended by a hook at the top of the receiver.

My firſt experiments were on frogs. Seven were confined in one receiver, and air left, equal in bulk to a pound of water. In half an hour, the water began to riſe above the level of that without, evidently proving that the elaſticity of the internal air was affected. The aſcent continued until all the frogs were dead or dying. The water had riſen eleven lines. I repeated the experiment with four frogs confined in the receiver. When the whole were dead, the water had aſcended ten lines. The elevation was one line higher in an experiment with two frogs. It was nine lines with only one frog.

I made ſimilar experiments on newts, reſerving the ſame quantity of air in the receiver as for frogs. The death of eight newts raiſed the water ſcarcely an inch; of four newts, nine lines; of two, ſix lines; and of one, five lines. The elevation of the water, therefore, diminiſhed with diminiſhing the number of newts.

By the death of eleven leeches, the water roſe five lines and a half, and by the death of three only one line.

Several naturaliſts have remarked how much ſmall animals injure the elaſticity of the air. I had alſo obſerved this in birds: Veratti's reſearches

ſearches were chiefly directed to them: but nobody that I know of has made experiments on an animal partaking both of the nature of a bird and a quadruped, although it is not properly either the one or the other; I mean the bat; an animal ſo diſguſting and forbidding in appearance, but at the ſame time as perfect as other animals, and the connection of quadrupeds with birds. Their ambiguous nature made me deſirous of forcing them to reſpire the ſame air in cloſe veſſels: but it firſt occurred to try how long they could ſupport a vacuum. How much ſooner did they die than the cold blooded animals? Five bats, ſucceſſively ſubjected to the experiment, did not live three minutes. They were of that ſpecies called, by M. D'Aubenton, the horſe-ſhoe bat, from the circular line on the noſe.

Though they died ſuddenly in vacuo, there was a limited proportion in confined air. Four, cloſed up in a veſſel, lived ſcarcely an hour; two, an hour and a half; one, lived almoſt three hours. The water of the veſſel, in the firſt caſe, aſcended an inch and ſeven lines; in the ſecond with two, an inch and three lines; and in that with one, eleven lines.

My experiments were extended to ſeveral reptiles; namely, vipers and ſome land ſnakes. Both theſe ſpecies having died in cloſe veſſels; the water roſe to a certain degree as the number was greater.

The

The greateſt elevation occaſioned by the death of three vipers, was an inch and three lines; and the leaſt elevation, by the death of one viper, ſix lines. The death of one ſnake raiſed the water four lines; and of five ſnakes, an inch and ſeven lines.

The ſame is the caſe with ſmall lizards and fiſhes. The elevation of the water is in proportion to their numbers.

I have repeated all theſe experiments in every ſeaſon, and have uniformly ſeen the water within the receiver riſe; with this ſingle difference, that the elevation is more accelerated in warm than in cold weather, as well as the death of the animals ſubjected to experiment. I have alſo conſtantly obſerved, that elevation is ſo much the leſs as the animals are ſmaller. Vipers, ſnakes, and bats, raiſed it more than frogs, newts, and lizards; and lizards raiſed it more than leeches. This even takes place according to the difference of ſize in the ſame ſpecies. The death of a barbel, weighing a pound, raiſed the water above an inch: that of one weighing only two ounces, did not raiſe it two lines. It ſeemed to me, that, in conſequence of this proportion, the death of the ſmalleſt animals ſhould raiſe it very little, or hardly at all: which would happen if their death was not occaſioned by the diminiſhed elaſticity

elaſticity of the air; becauſe the water in the receivers, remaining at its original level, ſeemed to indicate that the elaſticity underwent no alteration. To diſcuſs this fact was moſt important.

I began with the larvæ of large fleſh flies. Thirty, extremely minute from being lately hatched, were put into a very ſmall receiver. I left them on the fleſh where they had been depoſited by the mother. They lived only ſeven hours in the receiver, and the water roſe half a line. The ſame experiment was repeated on fifteen more: I could ſcarcely perceive the water elevated above the level; and it undoubtedly was not when eight larvæ were taken, though the whole died.

The larvæ of common flies exhibited nearly the ſame phenomenon. The water roſe one third of a line, when the number was great; when ſmall, the riſe was not ſenſible.

The death of ſeven earth worms did not raiſe the water. The larvæ of nymphs and gnats had only an inch of air: they died in a day; and though ſome hundreds in number, the water ſtood at its original level in the receiver, after they died. The death of five rat-tailed worms did not ſenſibly alter the level; but the death of a greater number occaſioned a perceptible elevation.

Some ſtagnant waters are full of a kind of minute animals, called water lice, or fleas, by natu-

raliſts. They are in conſtant motion, darting through the water in various directions (1). Several thouſands lived two days and ſome hours in a receiver, and died without any ſenſible elevation of the water. Neither could I perceive any elevation of water full of animalcula, which died in two weeks.

My experiments on many inſects which undergo no metamorphoſes, as ſpiders, ſhell and naked ſnails, millipedes, and on others that change their form, as caterpillars, chryſalids, and nymphs, demonſtrated, that the death of a great number raiſes the water a little, but not at all when they are few.

The facts we have obtained are now enough to decide on the object of our enquiry; eſpecially if compared with thoſe of Sig. Cigna and Veratti (2). Two principal reſults ariſe: Firſt, that

(1) The author evidently means various ſpecies of *monoculi*.—T.

(2) In the fourth volume of the *Acta Bononiæ* is a memoir concerning the effect of confined air, (in which a candle has burnt,) on animals, and ſome other experiments on confined air. This memoir has probably been overlooked, though the *Acta* are frequently quoted. The author does not aſcribe the death of animals 'to the diminiſhed elaſticity of the air, but to ſome change which it undergoes from its natural ſtate, by frequent inſpiration and

that there are many animals whoſe death in cloſe veſſels does not leſſen the elaſticity of the air. Secondly, when it is diminiſhed, it is very little. The firſt reſult is unfavourable to thoſe who aſcribe the death of animals to the diminiſhed elaſticity of the air. It cannot be attributed to this: for, if animals die, in many caſes, without the air ſenſibly loſing its elaſticity, we muſt conclude that their death has another cauſe. And I doubt very much whether in any caſe diminiſhed elaſticity has occaſioned death, becauſe it was ſo little. By Veratti's experiments, it appears that the barometer did not fall much more than an inch at the greateſt alteration. According to mine, the water in the receiver ſometimes roſe a few lines, ſometimes near an inch, and at moſt an inch and ſeven lines; that is, one fourteenth of an inch, and ſeven fourteenths of a line of mercury. But we know that in changes of weather, there is a greater difference in the weight of the air. The mercury in the barometer ſometimes falls more than an inch, and falls very faſt, eſpecially in ſtorms, without affecting animals;

' and expiration, ſo that the pabulum is conſumed by the ' animal, or the air is decompoſed, and becomes unfit to ' ſupport life.' Thus, this author is very near the true theory of reſpired air. *Thomæ Laghii de Animalium in aere intercluſorum interitu.*—T.

otherwiſe, neither cold nor warm blooded animals would live in ſafety on mountains where the mercury falls lower than in barometers placed in cloſe veſſels (1). Animals can not only live in air which has loſt its elaſticity to ſuch a degree that the barometer falls ſome inches, as on the tops of lofty mountains, but in air, if it is renewed, where the barometer falls to leſs than half its natural height. Such is the ingenious experiment of Sig. Cigna. This acute experimentaliſt confined two ſparrows in the receiver of an air-pump: one was left at liberty: the other put into a glaſs bottle, around the neck of which a very large empty bladder was tied. Then he began to exhauſt the air until the mercury, which ſtood at twenty-ſeven inches and a half in the barometer, fell to nineteen. As much air was then returned to the receiver as depreſſed the mercury two inches within. In a ſhort time afterwards, he drew the ſame quantity from the receiver,

(1) However, it is certain that animals removed from plains to mountains ſuffer conſiderably, though they may naturally live in ſafety on the ſummits of the higheſt. M. de Sauſſure aſcended to the top of Mont Blanc; the barometer fell to $16\frac{1}{11}$ inches His reſpiration was ſo much affected and he was enfeebled to ſuch a degree, that with great difficulty he could buckle his ſhoe. *Senebier, Memoire ſur la vie et les ecrits de Horace Benedict de Sauſſure.*—T.

receiver, which he ſoon returned, and continued this alternate exhauſtion and return half an hour. The ſparrows were always kept in air rarified ſo much as to balance only eight inches and a half of mercury, or at moſt ten and a half. But the ſparrow at liberty enjoyed the benefit of reſpiring renewed air; while that confined always reſpired the ſame. It expired ſoon after removal from the veſſel, whereas the other came from the receiver in perfect health (1).

Boyle tells us that animals periſh in condenſed air rendered much more elaſtic than the atmoſpherical. I have often repeated the experiment, condenſing the air ſometimes twice, thrice, or even more, than its natural ſtate; and with him have obſerved that the air, thus rendered moſt elaſtic, kills animals ſlower, but they periſh irrecoverably.

Thus it is experimentally demonſtrated, that the diminiſhed elaſticity of the air is not, and cannot be, the efficient cauſe of animals dying in cloſe veſſels. We have next to enquire whether their reſpiration contributes towards their death; which is the hypotheſis now to be diſcuſſed, and will be the ſubject of the following chapter.

(1) Miſcell. T. 2.

CHAP. III.

WHETHER THE RESPIRATION OF ANIMALS IN STAGNANT AIR OCCASIONS THEIR DEATH.—WHY THE DEVELOPEMENT OF SEEDS AND EGGS IS, IN CERTAIN SITUATIONS, PREVENTED BY CONFINED AIR.

THREE points are to be examined in diſcuſſing the firſt queſtion; Whether the death of animals in confined air is occaſioned by their reſpiration? 1. If we actually find exhalations in cloſe veſſels where animals have died: 2. If the exhalations occaſion their death: 3. Suppoſing they are noxious, how do they operate?

With regard to the firſt article, Sig. Cigna, the learned illuſtrator of this theory, has proved the reality of the exhalations of reſpiration, by the fœtid odour we are ſenſible of on opening the veſſels where animals have died, and a ſort of vaporous pellicle covering the internal ſurface. In my experiments, I have almoſt uniformly obſerved this pellicle over the internal ſurface of the veſſels where warm blooded animals, as birds, rats, or bats, have died, but never have remarked it with cold blooded animals. Something

thing indeed was perceptible on opening the veſſels; and the odour was certainly foetid or cadaverous. I have been ſenſible of it in all my experiments, which are very numerous, even in thoſe made on the ſmalleſt animals: ſo that the exiſtence of this vapour is not to be doubted, though it is not always viſible; either from the ſmallneſs of the quantity, or becauſe it is of a dry nature which may prevent it from appearing under the form of an aqueous veil.

It is inconteſtible that theſe exhalations are the real cauſe of the death of animals. Sig. Cigna endeavours to prove it by recurring to the reſemblance we remark between the phenomena exhibited by fluids evaporating in confined air and thoſe ſeen in animals reſpiring in cloſe veſſels. The evaporation he has obſerved continues longer in proportion to the ſize of the veſſels, and fills them ſooner as the air is more rarified. Animals are alſo ſubject to theſe two conditions; they live longer as the quantity of confined air is greater, and periſh ſooner as the air becomes rarer (1).

Collecting ſome of the reſults hitherto given, and connecting them with thoſe that I ſhall afterwards eſtabliſh, it ſeems eaſy to prove this fact. We have ſeen that two circumſtances accelerate

(1) Lib. cit.

the death of animals: the increaſed heat of the atmoſphere, and the number of animals confined. The exhalations are in theſe caſes more copious: reaſon perſuades us of it; and it is confirmed by the more foetid odour on opening the veſſels. As we cannot aſcribe the death of animals to the diminiſhed elaſticity of the air, and as no other cauſes appear, why may we not recur to the exhalations now become more denſe and active? Beſides, as we ſhall ſee, animals die ſooner in cloſe veſſels where there are ſome already dead, becauſe the exhalations are more abundant. By this means, we comprehend how animals confined alone, or in ſmall numbers, ſurvive much longer than where there are many, and why they live longer in cold weather, for in either caſe the quantity of the vapour is leſs. For the ſame reaſon, their lives will be abridged in proportion to the ſmallneſs of the veſſels; as the vapours become denſer from the little ſpace they have to ſpread.

With all theſe facts, we cannot affirm that diminiſhed elaſticity of the air occaſions death, and not the exhalations of reſpiration, or ſome material change which the air undergoes; particularly as, by continued inſpiration and expiration, it will ceaſe to be the ſame for want of free circulation, and will loſe the *pabulum*, which is a ſubſtance or quality known only by name, but

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on which every one makes the life of animals depend, either by being decompoſed or corrupted, ſo as to become unfit for reſpiration. But the ſlender ſupport of theſe reaſons is demonſtrated by animals themſelves, for they die in ſituations where the communication of the internal and external air is uninterrupted. When ſeven frogs had died in a receiver, I opened the ſtop-cock, and gave admiſſion to the external air to balance that within; in an hour, other two frogs were confined, the ſtop-cock remaining open, but they died in leſs than an hour and a half. Further, the frogs in the receiver periſhed ſooner, as the number of dead frogs was greater, although the diameter of the opening above was at leaſt two lines. Several birds, reptiles, and ſmall quadrupeds had the ſame fate when forced to remain among dead animals, though the receiver was open above. As the communication of the internal with the external air was uninterrupted, the imagined alteration or corruption of the air cannot exiſt, and death is certainly occaſioned by the exhalations of reſpiration, ſince we have ſeen that it happens when the top of the receiver is open, as part of them can then eſcape.

It may be remarked, in paſſing, that the death of animals in an open receiver deciſively proves that the diminiſhed elaſticity of the air does not kill

kill them. I made a new experiment to aſcertain poſitively whether they were deſtructive. The opening of the receiver was enlarged, and the exhalations were tranſmitted into a veſſel applied to it. In the veſſel were confined two ſwallows, and it was completely ſecured by a wooden ſtopper, well fitted. I preferred ſwallows as eight had previouſly died in the receiver. The influence of the exhalations was ſuch, that both died in a quarter of an hour, though two confined in a ſimilar veſſel lived fully two hours.

The experiment was diverſified, by collecting various quantities of vapour in the veſſel. Animals have always lived in proportion to the quantity. Long after the vapours are confined, they retain their deſtructive property. Whatever animals have afforded them makes little difference in their influence: they are equally fatal to all others. Exhalations from birds kill quadrupeds, and thoſe from quadrupeds are deſtructive of birds.

To terminate the inveſtigation, I made the following experiment. Many animals having expired in a receiver during very warm weather, I opened the hole at the upper part, and preſented a bird to the very fœtid vapour which eſcaped, ſo that it was forced to inſpire the mephitic air; this and every one treated in the ſame manner periſhed.

Although

Although I conceive it impoſſible that any animal can live in confined air, if the veſſels are very ſmall, it is certain that ſome ſurvive much longer than others. Cold blooded animals generally exiſt longer than warm. In the ſame air where a newt or a frog will live a day, a ſparrow, a bat, or a rat, frequently do not live an hour. There is even a ſort of gradation among cold blooded animals. A newt lives longer than a frog, and a frog ſhorter than a leech. This alſo ſucceeds with inſects. In the opinion of ſome, it is not difficult to aſcribe a reaſon for the diverſity of theſe phenomena. Cold blooded animals are not only more tenacious of life, but they have incomparably leſs neceſſity to reſpire than the warm blooded have. How much longer will a frog, a viper, or a toad, ſurvive in vacuo than a bird? Not being under the ſame neceſſity to reſpire as the others, they will abſorb leſs of the exhalations in equal time. Why then is it wonderful if they live longer? Doubtleſs ſome analogous reaſon occaſions the difference of time that cold and warm blooded animals can live in cloſe veſſels. Experiment proves that frogs in vacuo die ſooner than newts; and I have found the ſame on keeping both immerſed in water. We may reaſon in a ſimilar manner on inſects, not to ſpeak of their different conſtitution, which may produce diſcrepancies.

It remains to inquire how the exhalations of reſpiration are noxious to animals: and here in particular has Sig. Cigna diſplayed his abilities. According to him, they induce death by irritation of the bronchiæ and lungs, forcing them to contract and corrugate, and thus prevent the admiſſion of new air. Thus, in his opinion, animals in air infected by their breath die by ſuffocation. He endeavours to prove that this is actually the cauſe of death, from the various ſymptoms obſerved in the reſpiration of confined animals. Reſpiration becomes more frequent and fainter when the exhalations begin to collect, becauſe an equal quantity of air inſpired containing more, it obliges the animal immediately to diſcharge it. As they continue to increaſe, reſpiration alſo continues frequent, but becomes more laboured; and ſoon exhauſts the animal if confined in air where other animals have died. All this proves, in Cigna's opinion, that the exhalations injure the animal, by contracting the organs of reſpiration by their irritating influence, and thus obſtruct admiſſion of the air.

In my numerous experiments, I have ſeen the ſame ſymptoms of injured reſpiration. They are manifeſt in warm blooded animals, and eſpecially in birds. In cold blooded animals, they do not ſo evidently appear, but are very perceptible on changing animals from open to confined air

air previouſly vitiated by exhalation. Sig. Cigna had experienced this. He confined a frog in a receiver where five or ſix were already dead: the frog inſtantly became agitated, and leaped violently againſt the ſides of the receiver; it was immediately affected by a frequent and laboured reſpiration, which gradually became more painful, and ſoon ended in death. Therefore I fully coincide with the Turin profeſſor, that reſpiration is injured; but I cannot admit that ſuch a contraction of the organs enſues as to kill the animals by ſuffocation. Several frogs will live a long time in a capacious veſſel. Their vivacity remains more than a day if the weather is not warm: after this they become ſluggiſh, and ſwell exceſſively. If males, the two veſicles on the ſides of the head increaſe, and their inflation is ſo great as to prevent the frogs from ſinking, and keeps them always on the ſurface of the water. Having remained ſome time in this ſtate, the frogs expire. On opening them, we diſcover that the ſwelling proceeds from inflation of the lungs, which cannot poſſibly be more diſtended. The ſame inflation is found in thoſe of toads and frogs dying in this manner; and they, too, are neceſſitated to ſwim. But the lungs are ſo far from being rigid and contracted, that they are much dilated and conſiderably charged with air. For this reaſon, then, and for others of which I

ſhall

ſhall afterwards ſpeak, I cannot admit that animals die from obſtructed reſpiration. Several vipers and frogs were confined in very ſmall veſſels where many animals were already dead, and an equal number put at the ſame time in vacuo. It is incredible how much ſooner the former died; ſome did not ſurvive a minute; but thoſe in vacuo were alive at the end of ſeveral hours. It was not ſuffocation, therefore, or any obſtacle to the air entering into the lungs, that occaſioned death, otherwiſe the animals would have lived much longer, at leaſt as long as thoſe in vacuo.

Frogs were alſo forced to remain under water, and prevented from riſing to reſpire at the ſurface. I have tied the origin of the lungs in ſuch a manner that the air could not enter. Some I have deprived of the lungs, and confined along with other frogs in a ſmall quantity of air very fœtid from the exhalations of reſpiration. The laſt expired in a few minutes, ſometimes in one, even in leſs; while thoſe prevented from reſpiring by immerſion in water, having the lungs tied or cut out, have lived, as thoſe in vacuo, for ſeveral hours. I have found the ſame with toads and water ſerpents. Since the death of all theſe ſpecies, by privation of reſpiration, has been without compariſon later than the death of the ſame animals killed by the exhalations of reſpiration, we muſt conclude that

theſe

theſe exhalations do not kill by ſuſpending reſpiration, but that they are one of the poiſons moſt fit to deſtroy life, acting as immediately as any other poiſon, and even almoſt inſtantaneouſly deſtructive when collected in a great quantity. This poiſon penetrating the body by means of reſpiration, when animals inſpire the air, will cauſe that laboured breathing they ſuffer; for more than probable, it makes a violent and painful impreſſion on the organs of reſpiration. At the ſame time, theſe are not the only vehicles for the poiſonous vapours. Earth worms, leeches, and ſome other inſects, which are not only without real lungs but alſo without ſtigmata or tracheæ, die in like manner with the reſt in confined air (1). It is neceſſary to admit, that the exhalations act upon them, either by inſinuating themſelves through the pores of the ſkin, by the alimentary canal, or perhaps by both. The deleterious

(1) Vauqueline has made ſeveral experiments on the duration of certain animals' lives in confined air. A female locuſt lived thirty-ſix hours in eight inches of common air; and a ſnail lived forty-eight hours in twelve inches. He concludes, from various experiments, that inſects and worms reſpire nearly in the ſame manner as warm blooded animals, and are capable of reſpiring vital air only. He found that worms died when all the vital air was conſumed, *Experiences ſur la reſpiration des Inſectes et des Vers. Annales de Chimie, Tom.* 12.—T.

leterious quality of the exhalations is ſo terrible to all ſpecies of animals, that its effects are extended to thoſe that never felt the lively impreſſions of the air, from their conſtant abode at the bottom of waters. The ſnails and little ſnakes of ſtagnant waters ſhewed me this. They crawled over the bottom of an open veſſel of water, without betraying any ſign of uneaſineſs: but, when confined in a very ſmall veſſel, they became reſtleſs, aſcended the ſides of the veſſel, and, contrary to their uſual habits, left the water, and ſoon expired. Thus the influence of the exhalations acts under waters, which is indubitable from the fœtor communicated to thoſe in receivers, as well as to what furrounds them whenever it is copious.

But how do theſe peſtilential exhalations occaſion the death of animals, if they do not kill by depriving them of reſpiration? It is not by coagulating, diſſolving, or decompoſing the fluids. The blood preſerves its original fluidity immediately after the death of animals in this way, and flows in the ſerum; and its globules retain their ſize and figure. Beſides, if the exhalations coagulated the blood, or contributed to render it more fluid, they could not occaſion ſudden death; for animals deprived of blood die where they abound (1).

I have

(1) De Fenomeni della Circolazione.

I have ſuſpected that this poiſon may be fatal by deſtroying the irritability of the muſcles, which is poſſible in two ways. The muſcular fibre may either become too relaxed or too rigid. No ſymptom of the muſcles being flaccid appeared, when the animals were taken from the confined air, but rather ſigns of rigidity. Such phenomena I obſerved in frogs: their hind legs and thighs were extended ſtraight out, as if they had been dried. On changing their poſition, or bending them, reſiſtance was felt; and their natural poſition was reſumed when at liberty. The muſcular ſubſtance ſeemed harder to the touch and under the knife. However I ſoon diſcovered that theſe changes in the fibre did not precede death, but followed it. If I took frogs dying, or juſt dead, from the veſſel, their limbs were not ſtretched, and the muſcles preſerved ſufficient pliability. On the contrary, extenſion and rigidity only took place when they remained ſome time in the veſſel. I have ſeen the ſame muſcular hardneſs and extenſion in the limbs of dead frogs continuing immerſed in water. Alteration in the muſcular ſyſtem by no means happens to all animals.

The following facts made me entirely renounce the idea of loſt irritability. The femoral muſcles of a frog diſplay great irritability when cut or pricked; they tremble and ſuddenly contract,

not only where the point of the needle or edge of the inſtrument touches, but far beyond it; and the vibration continues ſome time, though the ſtimulus which occaſioned it is removed. Having taken a frog on the point of death from a cloſe veſſel,—I will not ſay the muſcles retained their irritability as in a ſtate of health, or that the approach of death did not weaken it,—but I will affirm, that the vibration and contraction of the thighs, when cut and pricked, re-appeared, and even continued after the animal was entirely dead.

I abandoned the idea of loſt muſcular irritability: and, after deep reflection, it has appeared to me that the nervous ſyſtem is the part on which the exhalations act. And here are the reaſons which have ſuggeſted that opinion; they are ſubmitted to the judgment of the philoſophic reader. Convulſions commonly precede and attend the death of our animals: they are clearly manifeſt in frogs. Sometimes the whole body is convulſed, but particularly, and more violently, the limbs; and in theſe convulſions they die. As they reſiſt death longer in winter, the convulſions alſo continue longer. If taken from the veſſel before death, it appears that the ſeat of ſenſation has ſuffered. They are ſluggiſh and motionleſs; nor do they change their place when ſtimulated, though agitation is evident; the convulſions re-

appear;

appear; their lethargy augments, even when put in the open air; and they generally die. Independent of being ſufficiently clear in itſelf, I obtained the moſt ſatisfactory proof that theſe convulſions proceed from injured nerves. I wounded the muſcles of a frog that had not been expoſed to confined air, and theſe motions which ſtimulating ordinarily awakens were excited; but they were never convulſive motions. On the other hand, by touching the origin of the crural nerves with the inſtrument, which was an extremely fine needle, the limbs ſuddenly became convulſed, and then exactly in the ſame manner as in confined air. When I pricked the ſpinal marrow or the brain, convulſions were univerſal over the whole body. I have never been able to ſee either general or partial convulſions by wounding any part of the body but the nerves. Thus, from the convulſive ſpaſms in animals confined in cloſe veſſels, I am induced to ſuſpect that the pernicious vapour acts on the nervous ſyſtem.

I had ſtill one doubt to ſolve. I could not reconcile the almoſt immediate death of frogs, expoſed to the powerful exhalations of reſpiration, with the preſervation of life a long time, though deprived of the brain. In my work on circulation, it has been ſhewn that frogs will live ſeveral days, though the brain is taken away. But the doubt diſappeared on ſeeing their immediate

death, when, inſtead of wounding the brain, the origin of the ſpinal marrow was wounded. If a pin was introduced where it united with the brain, the frog in a moment died convulſed. Animals, as tenacious of life as frogs, die equally ſuddenly, by injuring the ſpinal marrow. Therefore it is not very extraordinary, that death ſo immediate is occaſioned by quantities of the peſtilential exhalations inſinuating themſelves into animated bodies, and not affecting one part of the nerves or another, attack the whole ſyſtem, and momentaneouſly deprive it of ſenſation.

But what can we ſay of the death of thoſe animals in which there is found no veſtige of nerves, as the eels of vinegar and the hoſt of infuſion animalcula? As theſe animals actually periſh, the analogy of ſo many others dying from the ſame cauſe renders it plauſible that the exhalations deſtroy them by contact. Conſequently we muſt admit, that, acting on their organs, they produce an effect ſimilar to that which they operate on the nervous ſubſtance of other animals, notwithſtanding their organic ſtructure may be without nerves, at leaſt ſo far, that we cannot diſcover them by the microſcope. Therefore they cannot evite the fatal influence of the exhalations; nor do I ſee how they ſhould eſcape, while unable to reſiſt the effects of the electric vapour and odorous effluvia. I beſeech the reader to conſider this hypotheſis

on

on the nervous ſyſtem only as a conjecture. I have not collected that aſſemblage of facts neceſſary to give it authenticity, nor have I had leiſure to enter into all the details, and make the moſt profound reſearches. I wiſh that others would undertake it; and I ſhall always entertain the ſame regard for thoſe who attain their purpoſe, whether they confirm or confute my conjecture; for I have no other view than the purſuit of truth.

In the firſt chapter, we have ſpoken of eggs and ſeeds which refuſed to germinate when confined in a ſmall portion of air. It is poſſible that this ſterility ariſes from the cauſes deſtructive of animals confined in cloſe veſſels. Compariſon will help to convince us. Butterflies, we have ſeen, do not come from chryſalids confined in ſmall veſſels. M. De Reaumur, I find, has had the ſame reſults, although the object of his experiments was different from mine. He hermetically ſealed glaſs tubes, four or five inches long, containing chryſalids, ſome had come from the cabbage caterpillar, and ſome from caterpillars that produce phalenæ: they conſtantly remained in their original ſtate, though confined above five months. They never unfolded; for, as he obſerved, they [*did not*] perſpire, and perſpiration is neceſſary for chryſalids to become butterflies. Theſe two facts he proved in a deciſive manner. When the chryſalids are in a very confined ſtate, as a tube of a few inches in capa-

city, the moiſture tranſpiring cannot diſſipate, on the contrary, it falls back on the chryſalids. Thus, in ſeveral days, they appear moiſt, and the humidity inſinuating itſelf into their bodies renders them diſeaſed. Therefore, the death of chryſalids enſues nearly from the ſame cauſe as that of animals in ſtagnant air. All this will properly apply to the eggs of inſects, and the ſeeds of plants. We know that eggs are not hatched until a certain degree of heat which promotes perſpiration. Confined in a ſmall veſſel, they reabſorb the exhalations that had before tranſpired from them, and theſe corrupt. The humidity covering the eggs, and ſometimes the ſides of the veſſel in conſiderable abundance, proves it. The ſame happens to vegetable ſeeds. I have often put them in cloſe veſſels, and, that they might germinate, in a little water. On taking them out, the part that had been expoſed to the air was viſibly covered with a humid pellicle.

For oppoſite reaſons, we ſee why eggs and ſeeds are developed in large cloſe veſſels; they are always in ſafety, for the vacuity is ſo great that the exhalations may diſperſe. For the ſame reaſon do butterflies come from chryſalids when the veſſels are capacious.

OBSER-

OBSERVATIONS AND EXPERIMENTS

ON

SOME SINGULAR ANIMALS WHICH MAY BE KILLED AND REVIVED.

SECTION I.

THE WHEEL ANIMAL.

In the hiſtory of infuſion animalcula, which I have treated ſo much at large, it has been ſaid, when once they periſhed from a defect of water or humidity, they could never again be brought to life, though moiſture was ſupplied, and their immerſion continued long. Of this, I have had the moſt convincing and repeated evidence in the experiments now to be narrated. But there are other animalcula which, notwithſtanding they inhabit infuſions, are ſo much diſtinguiſhed and privileged by nature, as to enjoy the advantage of real reſurrection after death. Such, among

others, are the *Wheel Animal*, the *Sloth*, the *Anguillæ* of tiles, and thoſe of blighted corn.

A microſcopic animalcule, inhabiting the ſand of tiles and ſewers, is called by naturaliſts the wheel animal. The abdomen is large, and ſituated towards the middle of the body: in the opinion of ſome, there is an heart. The poſterior part of the animal is provided with a minute trident, and the anterior divides into two trunks, bearing two moſt ſingular wheels at the ſummits. From theſe it has been named the wheel animal. One magnified is repreſented Plate 3. fig. 1. If the ſand we ſpeak of is put in water, and remains a certain time infuſed, the animalcule exhibits all its organs. If the water fails, the action of the wheels and heart ceaſes; the animal gradually loſes motion, and becomes lifeleſs: it contracts, grows very minute, and aſſumes the reſemblance of a dry emaciated ſkin, fig. 2. B. It is ſufficient to moiſten the ſand for its revival: then the body ſoon extends, the wheels and the trident appear, the heart is re-animated, motion is regenerated in the whole animal; it begins to ſwim, and exerciſes all the functions of life. That it has remained long dry in the ſand is of no importance. Leeuwenhoeck, who firſt had the good fortune to diſcover it, and from whoſe works I draw the chief part of what I relate, has ſeen wheel animals re-animated after being kept in

in dry ſand almoſt two continued years. With this naturaliſt, we muſt obſerve, that the trunks and wheels are not always completely diſplayed when the animal revives, but are ſometimes exhibited as in fig. 2. A. Such are nearly the three figures which Baker, after Leeuwenhoeck, has given in his Treatiſe, *The Microſcope made eaſy*, where he contents himſelf with repeating what that excellent microſcopiſt had written.

Though ſeveral naturaliſts have treated of the wheel animal, they ſeem to me to have done it but ſuperficially, and chiefly to have proceeded on the accounts of Leeuwenhoeck. Thus I thought my trouble would not be miſapplied in inveſtigating this intereſting ſubject, and illuſtrating it with additional facts; and I was particularly induced to it by the relation between it and the chief objects of thoſe tracts. I have, therefore, compoſed a brief and methodical hiſtory of this wonderful inſect from the materials which obſervation and experiment have afforded me; and it precedes the hiſtory of other animals enjoying the ſame privilege. When on the point of publiſhing the fruit of my labours, another of Baker's works written in Engliſh fell into my hands, where much is ſaid of the wheel animal (1). I rapidly perufed it; and at once perceiving that the

(1) Employment for the microſcope, London 1764.

the author propoſed to treat the matter *ex profeſſo*, intended to ſuppreſs in this work all that concerns the wheel animal, becauſe it would have been uſeleſs to treat of a ſubject already diſcuſſed by that learned obſerver. I ſhould certainly have done ſo had I not obſerved that Baker's obſervations were materially different from mine, becauſe his wheel animals were of another ſpecies. I therefore determined to publiſh my treatiſe, which was improved by it; its imperfections leſſened; and new important matter added. This will appear from many parts of my obſervations and experiments which I proceed to relate, beginning with a fact which may be patent to every one. I examined the ſand from a ſewer about three hours after it had been put into water. It was not difficult to diſcover the objects of my ſearch. The firſt drop, a mixture of ſand and turbid matter, when preſented to the microſcope, exhibited three living beings, which I immediately recognized as three of Leeuwenhoeck's wheel animals. On the anterior part of the body was a horn; the body ſwelled towards the middle; and the poſterior part was terminated by three points; but the anterior had neither trunks nor wheels, and the animals were nearly as in fig. 2. A. The body is tranſverſely annulated, and longitudinally radiated with ſome parallel prominent rays, fig. 3. The indiſtinctneſs of the rings and lines renders them

them difficult to be found; and one muſt be accuſtomed to obſervation, and have an acute eye, before he is able to ſee them. A ſmall longitudinal faſcia, covered with ſpecks, is obſcurely ſeen in the middle: and above it a circle, more viſible, formed as it were by two C's touching at the extremities. The origin of a little canal is ſeen at the upper part of the circle, A. B. fig. 3.

The animal being very flexile, it aſſumes ſome extraordinary ſhapes in its progreſſion. Sometimes extending, it becomes very ſlender, then it contracts into extreme corpulency. Sometimes the anterior part is contracted and concealed in the body, or the ſame happens to the poſterior part. Some of the body will be inflated, while the reſt is flaccid, or it will exhibit other motions as eaſy and ſingular to behold as they are difficult to deſcribe with preciſion; and all theſe remarkable figures ſucceſſively follow, though the animal remains ſtationary.

The wheel animal uſes the following method to tranſport itſelf from one place to another. It fixes the extremity of the tail to the plane which it intends to traverſe: then it extends the whole anterior part of the body. While in this ſtate, the animal detaches the tail, and, by contracting the poſterior part toward the anterior, it advances. The extremity of the tail is again fixed to the plane: the body is extended as before, the

tail

tail is detached; and, by contracting the anterior part, a new ſtep is made. The operation is repeated, and the animalcule paſſes ſo actively along as ſoon to traverſe the field of the microſcope.

This method of progreſſion, by means of contracting and extending the body, is common to many inſects, but eſpecially to apodal vermes, as is well known. A circumſtance peculiar to the wheel animal, is fixing itſelf by the point of the tail, which is eſſential to its regular progreſs, that without this precaution, it would have no motion except contortion or undulation. When the animal has found ſome points of ſupport, and is fixed by the tail, as to a centre, it frequently ſtops for ſome time, and ſtretches out the fore-part, as if examining around which way ſhould be taken; then, ſuddenly detaching itſelf, it advances in a given direction. In Leeuwenhoeck's opinion, the wheeler fixes itſelf by the three points terminating the tail. At firſt, I likewiſe thought them all neceſſary; however, with more attention, I perceived that the middle point only was uſed. To ſee it diſtinctly, the drop muſt be thin and tranſparent, and free of ſand; then we eaſily perceive, that ſo far from the lateral points fixing to the place of poſition, they do not even touch it, but are at a conſiderable diſtance, and the middle point is the only fixing one. When viewed with

a powerful magnifier, this point ſeems compoſed of a number of infinitely fine ſimilar points, which are almoſt imperceptible, fig. 3. D, therefore, correctly ſpeaking, it is theſe points that fix the animal.

The three wheel animals, which I then obſerved for the firſt time, were not ſwimming, they crawled at the bottom of the drop. It was ſoon evident that this was their practice when the wheels were not in action; of which any one may be ſatisfied by putting a quantity of ſand mixed with wheel animals into a watch-glaſs half full of water: he will immediately ſee, that thoſe on the ſurface of the ſand crawl over it, and do not commit themſelves to the water. The ſame is the caſe with thoſe buried in the ſand, when by ſhaking they are brought to the ſurface.

The animals moved actively through the drop, ſearching as for food among the ſand with the anterior parts, but they never went beyond the confines of the fluid; on approaching the circumference, they inſtantly returned.—Motion became languid as the drop began to evaporate, and the languor increaſed ſo much as to deprive them of the power of changing their place; though it all dried up, they continued to turn about and ſtretch themſelves; ſuch motions were moſt conſpicuous in the head and tail, which proceeded from and re-entered the body, and were entirely concealed

concealed when the drop had evaporated. The appearance of the three wheel animals then changed, not only in the loſs of motion and all ſemblance of life, but, from a great diminution of ſize, they became three minute corpuſcula, ſo diſtorted that it was impoſſible to recogniſe them for what they had originally been, fig. 4. A. B. C.

They were about an hour in this ſtate of apparent death. I then put a drop of the ſame water on them that had evaporated. The reader may well conceive my attention in obſerving the reſurrection, which was ſucceſsful as I had anticipated. In a few minutes, the animals began to ſwell, and a point appeared in one part, D, fig. 4. The pointed part moved by reciprocal extenſion and contraction; and the oppoſite part, having alſo become pointed, began to move. Theſe points were the animal's head and tail, proceeding from the place where they had been retracted and concealed, on evaporation of the drop. The tranſverſe annuli, the longitudinal rays, the internal and external organs, all re-appeared. The three wheel animals ſoon reſumed their original ſize and figure; they traverſed the ſand, and ſhewed themſelves to be alive and moſt vivacious.

Diſcovering ſome more wheel animals in the ſand of ſewers, I repeated theſe experiments, and found

found that they always revived, independent of the time they had remained dry. There is now before me a remarkable inſtance.—Some ſand is in my poſſeſſion on which I made experiments near four years ago, and it has been kept dry in a ſmall glaſs bottle: when moiſtened, the wheel animals in it inſtantly revive. This agrees with what has been quoted from Leeuwenhoeck. Baker obſerved a fact little leſs worthy of notice. He wet the inſide of a glaſs, where wheel animals had been kept dry for ſome months, and he ſaw them recover their original vivacity. It is of no conſequence though they have revived oftener than once: the ſame ſand has been dried eleven times, and wet as often. I have uniformly ſeen them die as the water dried, and revive when the ſand was moiſtened.

However, theſe facts muſt be underſtood under ſome limitation. Though the animals do revive repeatedly, and even after remaining long dry, it is certain that the number revived always decreaſes in proportion to the time the ſand continues longer dry, and the times it has been wet for their reſurrection. It is true, I have ſeen their eleventh reſurrection. The firſt time, they were very numerous, but the numbers continually decreaſed, and at laſt became very ſmall. It ſhould be added, that, ſtill wetting and drying the ſand, none revived the ſixteenth time. It is

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the ſame with reſpect to the ſand remaining dry. I took a portion from roofs, at the time numerouſly inhabited by the animals; and I have preſerved it dry in a box for three years, only moiſtening it every five or ſix months for my obſervations, but the reſurrections are always fewer; and now, at the end of the third year, it is no exaggeration to ſay, one third do not revive. I have not extended the experiment further. However, it is undoubted, if the wheel animals grow rare in the ſame proportion as they continue longer dry, a period will finally come when none will revive.

The time neceſſary to accompliſh reſurrection is unlimited. Some begin to live four minutes after moiſtening the ſand; life then extends to more; and in half an hour, all are reanimated (1). I am ignorant what can occaſion this difference in the time neceſſary for reſurrection. It may either be, becauſe ſome are in parts of the ſand better moiſtened than others,—or, although the whole ſhould be moiſtened at the ſame time, it may happen that all the wheel animals are not of the ſame texture. In that caſe, the more denſe or

(1) Baker's wheel animals began to exhibit life only in half an hour. It would appear, that he ſpeaks of thoſe that were longeſt of reviving. This might ariſe from his experiments being made on a ſpecies different from mine.

or conſiſtent will more ſlowly receive the impreſſion of the water, and be longer of reviving; or ſome may be diſeaſed, and leſs fit for immediate reſurrection. I have not perceived any very ſenſible difference of time between the reſurrection of thoſe that have been dry ſome hours, and the reſurrection of thoſe that have been dry ſeveral days, months, or even complete years.

As I knew the influence of heat in reſtoring the life of animals and vegetables, the ſand was frequently moiſtened with warm water: and the animals revived ſooner than when it was wet with water at the temperature of the atmoſphere.

But there is one condition indiſpenſible to the reſurrection of wheel animals: it is abſolutely neceſſary that there ſhould be a certain quantity of ſand; without it they will not revive. Let us enquire further into this. One day I had two wheel animals traverſing a drop of water about to evaporate, which contained very little ſand. Three quarters of an hour after evaporation, they were dry and motionleſs. I moiſtened them with water to revive them: but it was in vain, notwithſtanding they were immerſed in water many hours. Their members ſwelled to thrice the original ſize: but they continued motionleſs. This circumſtance appeared to me the more extraordinary, as it was among the firſt times I had wet the ſand, and of all the animals I had experimented on, theſe two were the only ones that

did not revive. To aſcertain whether the fact was merely accidental, I ſpread a portion of the ſand on a glaſs ſlider, and waited until the numerous reanimated wheelers became dry in order to wet the ſand anew. The ſand was careleſsly ſcattered on the glaſs ſo as to be a thin covering on ſome parts and on others in very ſmall quantity: here the animals did not revive, but all that were in thoſe parts with abundance of ſand, revived. A difference ſo remarkable made me ſuſpect that, the ordinary dwelling of theſe animals being in ſand, a certain portion muſt be preſent to enable them to paſs from death to life.

However, to acknowledge the truth, though I did not at firſt adopt this conjecture, I could not diveſt my mind of it, as it ſeemed to be confirmed by facts; beſides, on recollecting the experiments before made on wheel animals, they had certainly always been in ſand. To corroborate or confute the fact, it was only neceſſary to repeat the laſt experiment; for, if thoſe that revived had been mixed with ſand at the moment of reſurrection, or, on the contrary, if thoſe without ſand did not revive, it would be complete demonſtration of what had been the cauſe, namely, that the preſence of ſand was eſſential to their reſurrection. On repeating the experiment, it conſtantly followed that the animalcula never recovered life unleſs in places where there was a quantity

quantity of ſand. One of my friends, an eminent philoſopher and an excellent microſcopical obſerver, has uniformly obtained the ſame reſult in his experiments (1). The Abbé Roffredi, a good obſerver, when incidentally ſpeaking of the wheel animal in the *Journal de Phyſique*, by the Abbé Rozier, mentions the like phenomenon.

To theſe, the following facts may be added. If the ſand containing wheel animals is ſpread out in ſuch a manner that a conſiderable quantity is in ſome places, much leſs in others, and very little in the reſt, and the whole then moiſtened; in the firſt caſe, where it is plentiful, the reviving animals will be numerous; where it is more rare, fewer will recover: And, in the laſt caſe, very few, or ſometimes none. If a dried portion of ſand, which was thick before, is ſcattered very thin and moiſtened, few wheelers will appear, though formerly numerous. Their reſurrection in a ſmall quantity is uniformly later than where ſand abounds; here it will be complete in four minutes: when there is little ſand, reſurrection will require nine or eleven minutes, and ſometimes more.

Have the animals not reviving from a defect of ſand, and reſembling globules floating in the water; have they loſt the reſurgent faculty, or do they reſume it on being ſupplied with their

(1) Il Padre D. Carlo Giuſeppe Campi di Milano.

native ſand? I have often taken the thin ſurfaces of ſand, wherein the wheel animals did not revive, and put them in the bottom of a watch-glaſs with water: but of twenty dead, ſcarcely one revived. It therefore ſeemed that privation of ſand deprived them of the innate faculty of reſurrection (1).

How can the ſimple defect of ſand produce ſo important an effect? What connection, what phyſical relation is there between ſand and the reſurrection of wheel animals? May not the cauſe of this phenomenon be entirely different, and the ſand only ſupply the place of ſome very ſimple external condition? When the animals periſh where there is no ſand, their bodies are expoſed to the immediate influence of the air on evaporation of the water: but they are ſecured from it, at leaſt in a much greater degree, if they die covered with ſand. May we therefore affirm, that the lacerating influence of the air, irritating and injuring the corpuſcula while ſtill humid, moſt tender and delicate, renders them incapable of reviving from the alteration undergone? My conjecture is founded on a fact evincing there are animals

(1) I have ſeen the common water ſnail revive after deſiccation a conſiderable time. When ſome did not recover, I made ſeveral experiments with different kinds of ſand, but all have yet been ineffectual.—T.

animals whoſe ſtructure is ſo fine and delicate, that, unable to bear the immediate impreſſions of the air, they always live under cover. Such are the *Miners*, a ſpecies of inſects ſo named from inhabiting the interior of the leaves of trees, where they live almoſt always concealed and protected from the influence of the air. The conjecture would perhaps require an experiment which I had not an opportunity to make. We ſhall immediately ſee, that wheel animals revive in *vacuo*. One might put ſome of thoſe ſurviving without ſand into an exhauſted receiver for the water to evaporate, and obſerve if they revived on being wet, which, according to my ſuppoſition, they ſhould, becauſe in that ſituation they could ſuffer nothing from the agitation of the air when the water evaporated (1).

We come to another inquiry more important than the preceding: I have hitherto ſuppoſed, that the wheel animals *periſhed* when the fluid dried. It is true they exhibit every appearance of death; the body is dry and disfigured; the

(1) Not only has the air a ſenſible effect on the tender bodies of animals, and they endeavour to eſcape from it, but there are ſeveral, even thoſe deſtitute of viſion, which cannot bear the influence of light, a ſubſtance infinitely more rare, and retreat from it as from a malignant agent. —T.

uſe and motion of the members are loſt. But this muſt be inveſtigated more profoundly, ſince it preſents the moſt paradoxical truth to be found in the hiſtory of any animal; and we cannot be too diffident and ſuſpicious of ſuch facts. Let us, therefore, inquire, if it is not poſſible that the animals, to all appearance dead, may preſerve ſome ſpark of life. And here let us recur to the analogies between large and ſmall animals. Cold, ſo injurious to inſects, renders thoſe lethargic during winter which it does not deſtroy: their torpor is ſuch, that they ſeem dead to the ſight and the touch: their limbs are ſtiff and contracted; their wings depreſſed, and their bodies emaciated. This we daily ſee in hundreds of inſects which we caſually find on the coldeſt days of winter, in the earth, the clefts of trees, or the holes of walls. In this manner does cold operate on animals poſſeſſing the higheſt rank in the animal ſcale. In the midſt of winter, we have found marmots ſo lethargic, that the flame of a candle burning their limbs could not awaken them, or recall the ſenſations of life (1). Terreſtrial and amphibious animals kept long in water exhibit the ſame appearances. Redi having immerſed flies in water an hour and a half, found them with all the appearance of death. Reaumur

(1) Buffon Hiſtoire Naturelle.

mur made the ſame experiment on bees. Their vindictive diſpoſition, when rudely handled, is well known. He left a whole ſwarm, I know not how long, in water, and found them ſo completely deprived of ſenſation, that he handled them at pleaſure: he took them from the water, put them on a table, and examined whether or not there were ſeveral queens. The ſame may be ſaid of the apparent death of frogs and newts; after ſome hours immerſion in water, their bodies become flaccid and drooping, juſt as happens in death.

May not the apparent death of inſects and other animals be ſimilar to that of wheel animals among ſand? But they preſerve a real principle of ſenſation and life, which the concurrence of certain circumſtances is required to unfold and render capable of animating the whole ſyſtem. If the air becomes milder, motion and life return in the animals torpid from cold. If the bees and flies that have been immerſed in water are expoſed to the ſunſhine, they ſoon begin to move, expand their wings, and take flight. Frogs and newts recover their natural vivacity after being dry a little time. Why then cannot we ſay there is ſome latent ſpark of life in the wheel animals, which the aid of water is required to diſcover?

Conſidering theſe facts, and allowing their juſt value, we cannot deny that there is a reſemblance between the ſtate of dry wheel animals, and that of the animals we have named, with reſpect to appearance, perfect immobility, and complete inaction of their whole members. But there is ſeen a moſt remarkable and ſenſible difference, which muſt create a great diſtinction between them. In animals torpid from cold, whatever is the agent depriving them of ſenſation and motion, it only does ſo by deranging the neceſſary harmony between the fluids and ſolids; yet it does not derange them ſo far as to deſtroy what conſtitutes their fluidity or ſolidity. The ſame harmony ſubſiſts in the inmoſt parts of the body. I have repeatedly opened newts, frogs, toads, torpid by cold, and apparently dead; and I have found, that notwithſtanding the blood did not circulate through the limbs, it continued to circulate in the large veſſels, though with extreme languor. If a greater degree of cold has ſtiffened the ſolids, if it has coagulated the fluids, then it is certain that the animals periſh. This, beſides having been found by others in many inſects, I have myſelf ſeen in the toads, frogs, and newts, of which I ſpeak (1).

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(1) It is difficult to allow this its full extent. Blumenbach had a frozen frog which revived. Volta had ſeveral

I have alſo found a remnant of motion in the heart and blood of theſe animals half drowned, and doubt not that it continues in bees and flies. But if they remain long in water, all internal motion is deſtroyed, and every hope of recovery is gone. It is therefore indubitable, that in theſe animals returning to life, the quality which conſtitutes the exiſtence of the fluids, or ſolids, is not taken away, nor is the harmony that reigns between them totally deſtroyed. How very different is it with regard to wheel animals. When moſt vivaciouſly traverſing the fluid, their body reſembles a thick jelly; the touch of the point of a needle is ruin and deſtruction. When dry, the ſolids are contracted and diſtorted, the whole body of the animal is reduced to a hard ſhapeleſs atom of matter; pierced by a needle, it flies in pieces like a grain of ſalt. How is it poſſible that this atom, whoſe ſolids preſerve no veſtige of their former humidity and pliancy, and where the fluids exiſt no more, how I ſay, can we ſuppoſe, that in this dry and disfigured atom, a principle of life remains? Does animation exiſt in a frog, a toad, or a newt, when as dry

veral frozen for months, which ſtill exhibited ſigns of Galvaniſm; and the author afterwards kept a number of the ſame animals two years torpid in ſnow. They were dry, ſhrivelled, and friable; but revived on the application of heat.—T.

dry and rigid as wheel animals are among the ſand? May we conclude, and conclude with reaſon, that in them and other reſurgent animals, life is entirely gone, not only becauſe the reciprocal actions of the fluids and the ſolids is deſtroyed, but becauſe the fluids are entirely evaporated, and becauſe dryneſs and rigidity has changed the natural ſtate of the ſolids? If we ſaw a ſtiff and contracted frog, toad, or newt, gradually revive when put in water, as we ſhould call it a real and abſolute reſurrection, ſo ſhould we call that which happens to reſurgent wheel animals a real and abſolute reſurrection alſo.

But it is time to reſume the hiſtory of theſe wonderful animalcula: We have already deſcribed their figure and properties, but we have not yet examined their organs ſeparately, which is eſſential before becoming well acquainted with both. By theſe I mean the heart, the two trunks, and the wheels, acting at the vortices. I could not properly do this before, becauſe it would have obliged me to deviate from my intended plan, which led to the relation of ſome facts following the order of time when they had occurred. It was in the proſecution of my obſervations, that the animal exhibited all the three organs. While animated, it frequently happens that the whole are not ſhewn at all during the time of animation, or diſplayed very ſlowly.

This

This is what I have obſerved in my wheel animals, and what ſome of my friends have obſerved along with me. I did not ſee the three organs till after twenty-one days examination. Let the reader figure a ſnail proceeding from its ſhell; it extends itſelf, and puts out the head and horns; then retiring within its habitation, it is contracted and conceals them within the body. In this manner he will ſenſibly conceive the motions of the two trunks and wheels of our animalcula. Thoſe then examined, and afterwards ſeen, did not always diſplay both the trunks and wheels at the ſame moment, but, like ſnails, ſometimes concealed the one and ſometimes the other, which happened whenever they contracted themſelves; and when remaining long extended, the trunks and wheels were kept out a long time alſo. The wheels receive this appellation in a very improper ſenſe, and by means of a ſort of latitude or accommodation.—In the tract on infuſion animalcula, I have treated at length of the minute, long, and ſlender fibrils proceeding from the edge of the mouth of many of theſe beings: I have ſaid they were in continual vibration; that they produced a certain vortex in the infuſions, which carried the corpuſcula feeding the animalcule to its mouth. The wheels of wheel animals are only two circular lines of ſimilar fibrillæ conſtantly in motion: they produce the ſame effect with the

the vibrating points or fibres of infuſion animalcula, forming two great vortices which convey the animal's food to it. A wheeler is exhibited, with the trunks extended, and the fibrils whoſe motion reſembles that of two wheels, and forms two vortices, fig. 5, plate 3.

When I ſay, the wheels of my animalcula are ſuch only in appearance, I do not mean that this will extend as a general rule to all. The optical illuſion indeed has been corrected by ſome naturaliſts, particularly M. Trembley and Bonnet; but it is certain, that the opinion of others is different. Leeuwenhoeck, that deep and acute obſerver of the moſt minute objects, actually calls them wheels, which revolve like thoſe conſtructed by mechanics. Baker, who is not inferior to him in accuracy of obſervation, and has ſtudied the wheels moſt attentively to diſcover whether they are truly ſuch or only vibrating fibrillæ, is more inclined to believe them wheels. What theſe two able naturaliſts have obſerved may very well coincide with my remarks, for their wheel animals have been ſpecifically different from mine. A ſingular aperture for a mouth ſituated between the wheels, a ſort of ring beneath it, a number of ſerpentine veſſels in the head, the periſtaltic motion of the inteſtines, the irregular agitation of a tranſparent fluid in every part of the body, a particular undulation of that fluid

fluid in the inteſtines and ſkin, were ſeen by the Engliſh philoſopher in his wheel animals; and although he has accurately deſcribed them, I have been able to find none ſimilar in mine. There is no doubt that I might have ſeen all theſe organs, both becauſe I uſed Cuff's microſcope, as Baker had done, and alſo ſome which were much ſuperior. With this different organization, it is not wonderful that his wheel animals exhibited another organ which I have not found in mine; that is, a pair of wheels proceeding from the two trunks, whoſe revolution produces the ſame effect as vibration of the fibrilli, and forms a moſt rapid current, which carries particles to the animal's mouth. It muſt be remarked, with Baker, that this apparent rotation is not always executed with equal velocity, nor in one direction. Sometimes it is very quick, ſometimes very ſlow; and theſe alternatives are either inſtantaneouſly or gradually effected. The animal, at one time, turns to the right or the left; and, after moving long at one ſide, it often interrupts the vortex and begins it in a part diametrically oppoſite.

Let us leave this brief digreſſion, and return to our animals. They no longer crawl at the bottom when the fibrilli appear, but ſwim through the whole fluid with the greateſt velocity. Examining them while in the act of ſwimming, I have

have often endeavoured to diſcover whether they ſwim by undulation of the body, or by vibrations of the fibrilli, which, beſides producing the vortices, may raiſe and conduct the wheel animal by their action againſt the water. But it has not been in my power to elucidate the fact ſatisfactorily; however, I ſhould incline to think they ſwim by means of the vibrating fibrilli, ſince they generally ceaſe when theſe are drawn within the body (1).

I have already ſpoke of a little circle ſituated towards the head, which appears like the junction of two C's by the extremities. This part is in conſtant motion by alternate contraction and dilation, while the animal forms the vortex, and the fibrils are extended. Both Leeuwenhoeck and Baker have obſerved it, and thought it the wheel animal's heart. Are we ſure it is this organ? The ſituation, figure, contraction, and dilation, concur, according to the Engliſh naturaliſt, to ſupport the opinion. But if it is a heart, it is a

(1) Some animalcula certainly may ſwim in this way; but the wheels of this animal in particular, bear ſo ſmall a proportion to the ſize of the body, that it is difficult to conceive their power to convey it along: beſides, its ſpecific gravity is greater than that of water. I ſhould imagine it more probable that the wheel animal ſwims by undulation.—T.

a voluntary muſcle, which beats at the will of the animal; that is, when it protrudes the fibrilli and forms the vortex: and this *ſpontaneity*, if I may uſe the expreſſion, has, before me, been obſerved by others. Are there animals whoſe heart beats by intervals, ſo that pulſation may ceaſe when the animal chuſes? Wheel animals ſometimes remain ſeveral weeks alive in water without making the vortex, conſequently without moving the heart. Is it poſſible that any animal can exiſt ſo long without pulſation of the heart, the animating ſpring of the whole machine? Theſe are two paradoxes which may be no leſs true than others more wonderful, ſuch as the reſurrection of the wheel animal. Though this particle may be thought a real heart, either from performing functions ſimilar, from its ſituation in the region of the breaſt, from contraction and dilation like another heart, ſtill theſe are not convincing reaſons, becauſe it may be an organ deſtined for very different purpoſes. I ought to ſay, as I think, that it is more natural to believe it to be an organ ſerving for the aliments; and that the contraction and dilation is for receiving the food and tranſmitting it to the ſtomach. Such an hypotheſis will eaſily explain why it is in motion only while the vortex is formed: it is becauſe aliment is then drawn to the mouth and tranſmitted to the body. If the part remains long motionleſs,

motionleſs, it is becauſe no food is taken, and this commonly happens when the animals, being in an unſuitable ſituation, languiſh and die; which is ſometimes the caſe with thoſe revived in ſand kept in cloſe veſſels. Then I have ſeen, that although the ſand ſwarmed with wheel animals during the firſt days, the number decreaſed, and thus continued to ſuch a degree, that in twelve or fifteen days the whole were dead. They appeared lifeleſs and disfigured at the bottom of the veſſels, and many were even reduced to nothing. During this period of diſeaſe, the vortex was ſeldom made; but it is almoſt always formed when we find them in ſewers, in pits or holes of rain water (1).

It is not an imaginary idea that this particle is an organ formed for the receptacle of the aliment, and to tranſmit it to the ſtomach. It is founded, 1. On obſerving in my wheel animals a kind of little canal united to it, fig. 3. B. 5. E. which, riſing towards the head, greatly reſembles an œſophagus. 2. This part is ſurely appropriated for that purpoſe in other aquatic animals, bearing great

(1) 'The heart of the wheel animal, which both Scopoli and Spallanzani affirm is at reſt when its filaments do not move, I have ſometimes ſeen in motion. It is rather a muſcle for deglutition.' Muller, Anim. Infuſ. p. 297.—T.

great relation to wheel animals, which may afford a ſtrong corroboration of the fact. Such is an animal often found on the tremella, ſhorter and a little thicker than the wheel animal. The poſterior part is provided with two diverging filaments, with which it fixes itſelf to any ſubſtance. At the anterior part are long ſlender fibrils, occaſioning a vortex in the water when the animal puts them in motion, fig. 6. There is no motion when the fibrils are at reſt. While moving, and during the continuance of the vortex, a particle, A, ſimilar in figure to that we ſpeak of in the wheel animal, is ſeen almoſt in the centre of the animal: it alternately contracts and dilates, but motion ceaſes with ceſſation of the vortex. This difference only is to be remarked; the particle of the wheel animal is formed by two ſemicircular cavities, whereas that in the tremella animal reſembles a bladder or folliculus. The particle and canal, towards the region of the head, are connected, as in the wheel animal, by a ſhort duct, B, terminating at the mouth of the animal, and at the oppoſite extremity it enters another folliculus, C, which not only moves with alternate contraction and dilation, but undulates like a wave at reſt. This folliculus is preciſely the receptacle of the aliments. It is always full of a yellowiſh green matter, which, from time to time, proceeds from

the poſterior part by means of the undulating or periſtaltic motion. But we not only obſerve the food diſcharged from the body, we ſee it enter; that is, fragments of the tremella are ſeen conveyed by the vortex to the animal's mouth, infuſion animalcula of various ſizes, and particles of other ſubſtances. Some of the moſt minute enter the origin of the œſophagus, traverſe the moving particle, and arrive at the paſſage for the receptacle of the aliment (1).

The ſame thing is obſerved in another animal of the tremella, which is mentioned in the tract on infuſion animalcula, pl. 1, fig. 10, B. The moving particle of this animalcule, for it has one as well as the wheel animal, contracts and dilates, while the aliment, collected by the vortex, paſſes from the œſophagus to the ſtomach. Thus we ſee a particle in theſe animals, which, in ſituation, figure, and motion, reſembles a heart, although it is not one, but an organ deſtined for the uſe of the aliment. For theſe reaſons do I aſcribe the ſame uſe to that of the wheel animal.

If I am right, the wheel animal has no heart: we ſee no other part, no other organ, that

(1) This ſeems to be Muller's *Vorticella Furcata*, p. 299. There is no figure of it among the plates, for which a reaſon is given in a note. I do not remember to have met with it.—T.

that can merit the name. If we may judge of this by the ſenſes, I ſay, it has none more than the two tremella animals, many infuſion animalcula, the prodigious number and variety of polypi viſible by the microſcope and to the naked eye, to omit many animals which it would be tedious to enumerate. I have never ſeen the appearance of circulation in wheel animals, infuſion animalcula, in thoſe of the tremella, or in polypi. Although Baker has obſerved the irregular agitation of a fluid in wheel animals, he ingenuouſly avows that he has never perceived any trace of real circulation; yet all feed, increaſe, and multiply, as thoſe animals which have a heart and circulation. Neither are they eſſential to the vital functions of many; for theſe, it is enough that there is a juſt equilibrium, a correſponding harmony between the fluids and the ſolids. The idea we form of a heart and circulation are particular notions derived from a definite number of animals, which demonſtrate the limits of our knowledge and underſtanding, and would ill apply if we meant to adopt them to the immenſity of models framed by nature.

The wheel animals inhabiting the roofs of houſes, towers, and other buildings expoſed to the inclemency of the weather, ſhould be of a conſtitution calculated to ſupport the ſevereſt effects of heat and cold. I put them to the teſt.

From a fewer in a ſouth expoſure, I took wheel animals' ſand, which had been expoſed to the heat of the ſun twenty-nine days in the middle of ſummer. The thermometer ſtood at 129°, 133°, 138°, in the ſunſhine; but the animals were not injured, for I had a great number very vivacious when the ſand was wet.

A little of this ſand was expoſed the whole ſummer, in very thin glaſs tubes, on the outſide of a ſouth window, where the reflection of a neighbouring wall excited extreme heat. During ſome of the hotteſt days, the thermometer roſe to 142°; but this did not injure the wheel animals. On wetting the ſand, they appeared with the ſame livelineſs and vigour, and in the ſame abundance, as in other ſand from the ſame place expoſed in a north window, and ſeldom or never experiencing the ſolar rays. Therefore it is evident, that the exceſſive heat of ſummer is not ſo prejudicial to wheel animals as to deſtroy the refurgent faculty.

But is it the ſame when revived? Is this degree of heat equally ſupportable? I have alſo expoſed the tubes with ſand and water, containing a great number of living wheel animals, in the ſame warm ſituation. The conſequence was very different. In half an hour, the heat of the ſun at 135° killed them. Thus, it is otherwiſe with wheel animals, dry and deprived of life, and when they are animated and in motion. I afterwards

wards saw that resuscitated wheel animals died at a much more gentle degree of heat, when exposed in the sunshine at 113°.

The heat of the fire has the same effect as that of the solar rays. Although the revived animals perish at 111° and 113°, if dry, they do not lose the power of resurrection at 144°. I could extend my experiments further with common fire than the sun. The heat was raised above 144°, to see whether dry wheel animals would revive; for it was probable there were limits here, and these I found at 153°. Sand exposed to this heat, presented few wheel animals; and exposed to 158°, none appeared. But there is one circumstance necessary to be remarked. The experiments were made dry, that is, keeping the sand two or three minutes exposed to the heat. The consequences were very different on using wet sand, immersing it two or three minutes in water warmed to that degree: then the animals did not revive after 131°.

It is not difficult to explain why the destruction of reanimated wheel animals is easier than when in their state of desiccation. The former are a kind of jelly, consequently most delicate. Their minute filaments are easily broken and destroyed by the penetrating power of heat, which cannot operate with such facility when they are dry: then the parts are concentrated within

themſelves, compreſſed and indurated. Beſides, in this ſtate, their globular figure preſents leſs ſurface to the action of the fire. The heat acts alone on dry wheel animals; when alive, it acts in conjunction with the water, which powerfully concurs in lacerating them and deſtroying their organization, from its particles being ſubtilized by the heat and rendered more active and penetrating. Thus it is that dry wheel animals can reſiſt the heat of warm water leſs than that of the fire itſelf.

Having ſeen the effect of heat on wheel animals, it was neceſſary to ſee the effect of cold. With this intent, I took the ſand of ſewers and from the hollows of the eaves and tiles, where they are found during the moſt intenſe cold of winter, when roofs are covered with ſnow and ice. The ſand, moiſtened with water, became ſo firm and connected by the cold that it was as hard as a ſtone; but the wheel animals were not injured. After melting this mixture of ice and ſand, a great many revived; however, their reſurrection appeared leſs immediate.

The greateſt cold of winter was 16°. I therefore determined to expoſe the wheelers found on roofs to a degree more intenſe: and taking ſome portions of frozen ſand from the bottom of a ſewer, I put them in a glaſs veſſel which was placed three hours in cold 11° below 0, obtained

ed by means of the mixture I have often mentioned. The revival of the animals, after the ice melted, proved that they had ſuffered no injury.

I next ſought for the reſult of the experiment inverſed; that is, what would happen on tranſmitting wheel animals, from the degree of heat at which they were animated, to various degrees of cold always more intenſe. One morning, ſome were taken in a watch-glaſs to a north window, where the thermometer ſtood at 25°, and I attentively obſerved what paſſed. When the water became ſo cold that the hand could ſcarcely be kept in it, the wheel animals interrupted the vortex and fell to the bottom, crawling languidly over the ſand. The water ſoon froze; then they moved with difficulty, and ſoon ceaſed. When more frozen, they contracted within themſelves, forming into globules which were clearly ſeen from the tranſparence of the ice. Thus they paſſed the whole day and following night, which was very cold. Next day, I removed them to a warm ſituation, to ſee whether thoſe in the ice, under the figure of globules, would recover when it melted. They did ſo, even when remaining longer in the ice, and when the natural cold was increaſed by means of fictitious to 11° under 0.

Reaſoning from the experiments by cold as from thoſe by heat, it might be inferred, that

revived wheel animals ſhould not ſupport the ſame degree as thoſe which are dead; if, on the other hand, theſe laſt facts did not evince that when the cold begins to act powerfully on thoſe revived, they paſs from life to death, as appears by the ceſſation of motion, their contraction and disfiguration, ſo that they become exactly as when evaporation of the water leaves them dry in the ſand. But I do not know that the action of extreme cold will deprive living wheel animals of life. It is certain that it could not deſtroy animalcula equally delicate, ſuch as ſome infuſion animalcula and the eels of vinegar. Yet, what is more ſurpriſing, the animalcula of infuſions and the eels of vinegar periſh with leſs heat than wheel animals: they cannot ſupport more than 111°. At that degree ſeveral ſpecies of beetles, chryſalids and caterpillars, periſh, as I before obſerved, though they can ſupport 34° below freezing. This ſhews that many animals in ſimilar ſituations can ſupport cold better than heat.

From the facts hitherto related, we collect that there are two principal cauſes deſtructive of the reſurgent quality in wheel animals, the want of ſand and of heat. Are there others alſo producing the ſame effect? I could diſcover it only by conjecture, and uſing the different methods prejudicial to the production and life of other animals, eſpecially of thoſe bearing the greateſt analogy

logy to wheel animals, as the animalcula of infusions. It has been proved that these are produced in vacuo. This seems an effectual method for preventing the resurrection of wheel animals, though we cannot deny that their resurrection is facilitated by the influence of the air. The principal results of repeated experiments, are, 1. Wheel animals revive sooner and in greater number in the open air than in vacuo. 2. Those that do not revive in vacuo, recover when put in the open air.

However much air may promote the resurrection of wheel animals, it is absolutely necessary for the preservation of their lives. When they revive in vacuo, or are put into an exhausted receiver, they die in a few days.

If wheel animals did revive in vacuo, though not so successfully as in the open air, it was very reasonable to suppose they would also revive in confined air, though it is one method of preventing the developement and of occasioning the destruction of other animals confined in very small vessels. I sealed up some with wet sand in vessels: they always revived very soon, and in abundance: they have even lived long, though, from the extreme smallness of the vessels, there could be very little air.

Wheel animals suffer from many fluids what they do not suffer from privation of air, or from air

air confined. The liquids injurious and advantageous to them are the following: Thoſe innoxious, I underſtand to be ſuch as either revive them or preſerve them alive when recovered; and of this nature are, pit, river, ice, ſnow, and rain-water, diſtilled water, that of ditches, marſhes, and pools, the fœtid water of mud and dunghills. With reſpect to the fluids pernicious, they are either thoſe impregnated with pepper, common ſalt, ſal-gem, ſugar, vitriol; thoſe in which are expreſſed the juice of onions, garlic, urine, ink, wine, verjuice, oil of olives, or nuts, brandy, vinegar, and the like. I never ſaw wheel animals revive when their ſand was put into any of theſe fluids, and all the revived animals put into them periſhed. Some ſtrong and penetrating odours have been equally fatal, ſuch as that of camphor. All the living ones long expoſed to its effluvia die, and theſe expoſed dry do not recover. The oil of turpentine produces only the firſt effect. But if the odour becomes more active, as by melting or burning the turpentine, the fumes prevent the reviviſcence of the animals. Both effects are produced by the fumes of burning ſulphur and camphor; only revived wheelers are deſtroyed by thoſe of leaf tobacco.

Reflecting on the experiments by means of heat, liquids, and odours, I have ſometimes doubted whether theſe three agents had deprived the

the animals of the refurgent property for ever, or if there was any reafon to hope that it might be recovered. In a being like the wheel animal, this hope did not feem chimerical, nor would it have been wonderful to fee it recover the faculty which it naturally poffeffes. I have preferved fand that had been expofed to heat, and from time to time wet it with pure water, and often obferved it. The fame has been done with fand expofed to liquids and odours, keeping it in the air and weting it with frefh water, that the noxious qualities which injured the wheel animals might be deftroyed. But the numerous dead bodies have never been re-animated by thefe methods.

The wheel animals, which fuggefted the opinion I have laid down, were for the greater part found in fewers and the hollows of tiles, among a fubftance which, for brevity, has been called fand, though, to fpeak more properly, it is a mixture of earth, fand, and the fragments of tiles. This fand, for I fhall continue to call it fo, is the abode of wheel animals, but in fome kinds they are much more numerous than in others. It is fingular, that if the fand is reddifh, it is almoft a certain indication, according to Baker, of their prefence. They are always inanimate when the fand is dry. By one accuftomed to obferve wheel animals in the dry ftate from evaporation of the water, they are eafily recognized

recognized when the ſand is preſented on a ſlider to the microſcope. Then they are in the figure of minute dry globules of a reddiſh yellow colour, which, by humectation, expand into ſo many animated wheelers.

Beſides roofs and tiles, particular waters contain them. Both Baker and myſelf have often ſeen them in ditches; and I have taken many from pools, marſhes, and even holes of ſtanding water.

The wheel animals of the earth are, in my opinion, the origin of thoſe of roofs: and this ſeems a neceſſary concluſion; at leaſt we cannot ſay thoſe of one roof come to another, which ſuppoſes a particular caſe: but to ſpeak generally, and conſider the origin of the matter, we muſt derive their origin elſewhere, conſequently recur to the waters. The manner in which they paſs from the earth to roofs, may be eaſily conceived; in their dry ſtate, the wind may tranſport them through the air, whenever their native element is entirely or partially dried up.

In my ſtudies on theſe curious beings, I have always reflected on a moſt important problem, to inquire into their mode of propagation. With this view, I have iſolated them in watch-glaſſes like infuſion animalcula, putting one in each; but I could never obſerve them propagate either by ſhoots or diviſion: though both ways are common

common among aquatic animals. Neither was it by a fœtus, but I had reaſon to think it was by means of eggs. When the animals had been ſome days revived, an ovular ſubſtance was ſeen in the body of the largeſt, Pl. 3. fig. 5. N: and when I happened to find them dead, they always had this ovular ſubſtance. But it had in general paſſed from their bodies into the glaſs without the means being known: at the ſame time with an important ſingularity: when entire, the iſolated animal ſwam alone in the fluid; but when the ſubſtance was broken, another wheel animal much ſmaller ſwam along with it. This made me ſuſpect, that the new inhabitant had come from the ovular ſubſtance, which, as other eggs, was broken for excluſion of the animalcule. One might ſuſpect, that it alſo was carried thither by the air; however, to aſcertain the fact, it was neceſſary to ſee the wheeler iſſue from the ovular ſubſtance, which, notwithſtanding all my care and attention, I could never accompliſh.

Baker's obſervations agree with mine, though he has not been more fortunate. He thinks wheel animals are oviparous, becauſe he has often obſerved a conſiderable number of gelatinous eggs of proportional ſize in the water along with them. He has alſo obſerved in a ſpecies of wheel animals, a little larger than the moſt common kind, an elliptic body, the figure of which very

very much reſembles the ſubſtance I have deſcribed: but he never had the ſatisfaction to ſee one excluded: nor has he ſeen a wheel animal come from the gelatinous eggs, though he kept them three years.

The learned Abbate Roffredi has enjoyed the good fortune denied to Baker and me, and put the finiſhing ſtroke to our obſervations. Incidentally ſpeaking of the wheel animal in Rozier's *Journal*, he ſays, in expreſs terms, he has ſeen it delivered of an egg, and an animal proceed from the egg. 'Leeuwenhoeck is miſtaken 'in thinking the wheel animal viviparous; and 'what he ſuppoſed excrements in the inteſtines is 'really an egg, which I have ſeen it produce, and 'often continued to obſerve until it was hatched.' If Roffredi's obſervation is correct, which, indeed, cannot be doubted, a fact in the hiſtory of the wheel animal is elucidated which ſhould deeply intereſt the naturaliſt.

The ſolution of this problem, added to my obſervations, diſcovers another important truth, namely, that wheel animals undergo no metamorphoſis. I have collected many minute ones produced in a watch-glaſs. They always continued growing; but when killed and revived their increment was ſlow; and quick, on the contrary, when they were kept continually wet. When full grown, each generally laid an egg, which

which produced another wheel animal. Thus I know, from the time of excluſion until maturity, they undergo no change. We cannot call it a metamorphoſis when they are produced, for then they have attained their greateſt perfection. The inſects that metamorphoſe never propagate their ſpecies until they become winged animals, that is, until they have acquired the laſt degree of perfection to which metamorphoſis will bring them.

Finally, my obſervations have ſhown me, that wheel animals are hermaphrodites in the moſt rigorous ſenſe. I have obtained the fifth generation from many eggs which were iſolated in watch-glaſſes to remove all ſuſpicion of copulation.

SECTION II.

THE SLOTH, ANGUILLÆ OF TILES, AND THOSE OF BLIGHTED CORN.

THE ſand of tiles, the mud of ditches and marſhes, which paſs in the vulgar eye for the vileſt of matter, are ſources of wonder to the philoſophic obſerver,

ſerver, from the rare and ſingular beings they contain. To the mud of ditches and marſhes we owe the cluſter, armed, bulb, funnel, and knotted polypus. It is there we find the freſh water worm, the boat worm, and the dart millepede, animals that have confounded the human mind, and created a new philoſophy. When the ſand of tiles is not the abode of wheel animals, it is not then the leſs famous or remarkable. An animal which revives after death, and which, within certain limits, revives as often as we pleaſe, is a phenomenon as incredible as it ſeems improbable and paradoxical. It confounds the moſt received ideas of animality; it creates new ideas, and becomes an object no leſs intereſting to the reſearches of the naturaliſt than the ſpeculation of the profound metaphyſician. But the celebrity of this ſand will increaſe, by learning that it contains other animals, which, like the wheeler, poſſeſs the property of reſurrection: ſo that we may almoſt ſay, all the animals living in ſand are immortal. There I have diſcovered two new ſpecies of animals, which I proceed to deſcribe. I lament that their rareneſs has prevented me from extending my obſervations as far as I could have wiſhed, or rather as far as the importance of the ſubject would have required.

On wetting wheel animals' ſand, I ſeveral times obſerved a yellowiſh animal three or four times larger

larger than a wheeler with ſix legs; but I paid no particular attention to it, ſuppoſing that it was ſome little terreſtrial inſect that had caſually fallen into the watch-glaſs where the ſand was kept. My reaſon for thinking ſo, was from always having ſeen it move obliquely and very ſlowly at the bottom of the water, as if unable to walk, and often ſupine, making great exertions to recover its natural poſition, but they were in general fruitleſs, as happens to many aerial and terreſtrial inſects caſually falling into water. At the ſame time, with more continued and careful obſervation, I recognized it as an animal really aquatic, and perceived that its awkward and laborious mode of progreſſion was from the ſmoothneſs of the glaſs ſlider on which it had been put for examination, and, when placed on ſand, that it had a regular progreſſive motion, ſlow indeed, and, compared with the wheel animals' motion, like the crawling of a tortoiſe. Thus to deſign it by ſome deſcriptive name, I called it the *Sloth*.

The whole body is granulated: the anterior part obtuſe: and the poſterior terminated by four hooked filaments, which ſerve for attaching it to any particular place. The limbs have ſmall ſhining claws, or nails, which, as far as one can judge, are of a corneous ſubſtance, the points turned towards the body, as we ſee in the re-

curved claws of ſeveral inſects. The corpulence of the ſloth, rendering it opaque, prevents us from ſeeing the internal organization. But we can perceive a ſmall elliptical ſpot in the middle of the body, which I ſuſpect to be the reſervoir of the aliments. In the anterior part is alſo diſtinguiſhed an internal lucid ſpot, ſmaller, narrower, and longer than the other, which I have ſometimes ſuppoſed the œſophagus. The figure of the whole is clumſy, and very much reſembles the teſticle of a cock. The ſloth is repreſented ſupine, fig. 7. pl. 3. the profile is ſeen fig. 8.

This animalcule forms no vortex in the water, which is not ſurpriſing, as it has neither the wheels nor fibrillæ of the animals that perform this operation. It appears that the wheel animal cannot advance a ſtep without fixing the trident to ſome adjacent ſubſtance; it is otherwiſe with the ſloth, for it often makes no uſe of its hooked filaments. It never ſwims; it is ſpecifically heavier than the water; thence it always turns round on the ſurface of the ſand, or amongſt it.

The phenomena of its death, from the want of water, and of reſurrection when water is ſupplied, are preciſely the ſame with thoſe of the wheel animal. Motion gradually ceaſes: the limbs are contracted and drawn entirely within the body, which diminiſhes very much, is completely dried, and aſſumes a globular figure, pl. 4. fig.

1. The reverſe ſucceeds when the ſloth is revived by ſupplying water. As the wheel animal can only revive a certain number of times, ſo it is with the ſloth. And, although ſand is neceſſary for its reſurrection, it does not appear ſo eſſential as for that of the wheel animal.

The degrees of heat, fatal to revived or dead wheel animals, are alſo fatal to ſloths; and the ſame may be ſaid of odours and liquors. Cold, however intenſe, does them no harm, and in this they likewiſe coincide with wheel animals.

Sloths are infinitely more rare than wheel animals: for five and twenty of theſe, four or five ſloths are hardly found. All are of the ſame figure, but not equal in ſize. I have iſolated many in watch-glaſſes, ſometimes with ſand and ſometimes with pure water, intending to diſcover their mode of propagation, but, inſtead of multiplying, all periſhed: ſome ſooner, ſome later, none ever attaining the ſixth day (1).

(1) This animalcule is uncommonly rare. I have never ſeen a deſcription of it by any other naturaliſt: and it only occurred to me in two infuſions. Muller's *Cercaria Hirta*, which he found but twice, ſeems to have ſome reſemblance, but he does not ſay enough of the properties to aſcertain it, nor does he remark any feet. I had mine from June until the following February. They were very

The third ſpecies of refurgent animals found in ſand conſiſts of certain minute eels, very like the anguillæ

very minute, not one tenth of the ſize of thoſe mentioned in the text, which, added to their rarity, was an abſolute bar to experiment. They had no terminating projections or hooks, nor were the different faſcia viſible. However, I ſometimes thought two hooks of very few were perceptible, but I could never be certain. The animal was perfectly opaque, which prevented any obſervation of the interior part. There was no ſand in the infuſion, which had been made of dried twigs eight or ten months before, and the ſloth's natural abode was among the particles of matter, eſpecially on the twigs. How it propagates I am ignorant: it did not multiply much.

I have diſcovered another animal which moſt probably belongs to this ſingular claſs. It was in an infuſion the ſame as the former, and there I had it from the beginning of May until the end of Auguſt. It then diſappeared, and I ſaw one in the ſubſequent February. In one other infuſion, I found two or three nearly the ſame. It bears the greateſt reſemblance to a moſt minute caterpillar, both in appearance and motion. It moves little, and then with great awkwardneſs and languor. The largeſt might be about one-third of a line in length, and perceptible by the naked eye, which could only diſtinguiſh a long white ſpeck, but few are of this ſize. Inſtead of ſix legs and four hooks, there are eight legs, the two laſt exactly like thoſe of a caterpillar. With the ſecond higheſt magnifier of my microſcope, I could obſerve each of the ſix

legs

anguillæ of vinegar. This ſpecies is much rarer, nor is it to be found on every roof. The head and

legs terminated by two long claws a little curved; whether the two hind ones were terminated by three, four, or five, could never be aſcertained; circumſtances were very ſeldom ſo favourable as to admit of the others being ſeen. In ſome, I thought the eye might be diſtinguiſhed, and an aperture for receiving aliment. There is a great difference of ſize. Moſt of them are of an opaque duſky brown, which ſeems their natural colour. Some are perfectly tranſparent, which I ſuppoſe are dead, for no motion has ever been evident, although the fact is not abſolutely certain.

I know very little more of the propagation of this animal than that of the former, except from conjecture. One night in May, I was ſurpriſed to ſee three large round ſubſtances in the body of a ſloth: it made no impreſſion on me at the time; but returning to obſervation, ſeveral nights afterwards, I ſaw the body of another, of the largeſt ſize, completely filled from end to end of the whole with ten large eggs. I do not think there was room for one more. Every moment I expected to ſee an egg proceed from the body. The obſervation was continued for hours, and during the next day. The ſame number remained: the animal was then obſcured by ſome particles of matter, and all endeavours to recover it ineffectual. Nor has my ſucceſs been greater with the few others appearing with eggs; for, notwithſtanding all poſſible care and attention, I never could ſee one produced. The ſloths containing them were all perfectly tranſparent and motionleſs. The

and adjoining part of the body are very tranſparent, and of a ſhining ſilver colour: the tail is the ſame, but the intermediate part is darkiſh and all granulated. The greater part of the tail is bent, and terminates in a very ſharp point. The head, on the contrary, is obtuſe; and a little below the extremity there is a mouth, which terminates a canal, apparently ſerving for an œſophagus, and traverſing the whole length of the body, pl. 4. fig. 2.

If

number contained from one to ten. I ſaw one with a ſingle egg, ſo large as to diſtend the body in the middle very much. Whether theſe ſubſtances are eggs or fœtuſes is uncertain: they are probably the latter included by a fine integument; and I rather ſuppoſe that the body of the mother burſts to give them an exit or immediately after they are produced. It is much more difficult to iſolate an animal that almoſt never moves than another that is continually traverſing a fluid. Sometimes I did ſucceed in iſolating ſloths with eggs in watch-glaſſes. When the young appeared, the parent was no longer viſible, at leaſt I could not find it; and they were remarkably ſmall in compariſon to others. There was no ſand where they were found; their natural abode was among the particles of vegetable ſubſtance. I did not ſucceed in reviving them; but their ſcarcity prevented me from making repeated and accurate experiments. This, if it is a ſloth, may be named *Tardigradus Octopdalis*, and that in the text *Tardigradus Italicus*.—T.

If the ſand is quite dry, they are ſeen motionleſs, dried up, and generally bent into a ſpiral. When conſiderably ſprinkled with water, they ſoon exhibit ſigns of life. The tail firſt commences a gentle motion, bending and turning in different directions: the head then moves, and afterwards the reſt of the body: ſo that the whole animal ſoon becomes animated. Whence it reſults, that the ſame degree of humidity is not required to animate this ſpecies as for the wheel animal and ſloth, which do not revive unleſs completely immerſed in water. The eels do not change their place: they only extend, contract, turn, and bend. If the ſand is thoroughly wet, their activity and rapidity of courſe is as great as that of the eels of vinegar. Provided they have water, they live long in watch-glaſſes: if there is ſand at the bottom, they ſeldom quit it, always moving about the grains and puſhing their heads among them, which would induce us to ſuppoſe that they do ſo in ſearch of food, for ſome more minute and delicate particles are tranſmitted by the mouth to the œſophagus. Notwithſtanding they have been long kept in glaſſes, I never ſaw them propagate.

When the water evaporates, they die; but they reſiſt death longer than wheel animals and ſloths. A ſmall degree of motion remains ſeveral minutes after evaporation. When dead, the fi-

gure of the body is changed: the length is contracted, and the breadth diminiſhed. They inſenſibly reſume their original ſize on humectation, and animation returns. There are conditions neceſſary for reſurrection: When the eels are in ſand, a quarter of an hour is ſufficient for recalling them to life; but in pure water, there is a great difference according to circumſtances. If only the firſt or ſecond time of revival, there is not much difference in the time required for reſurrection; but, in proportion as the number of reſurrections increaſes, the time neceſſary for revival always becomes greater: an hour at leaſt, and ſometimes more, is required for the fourth; for the fifth ſtill longer, and ſo on for the reſt. The frequency of reſurrection in pure water, as in ſand, is limited, like that of wheel animals and ſloths. The eels die for ever at the ſeventh or the eighth, or, at moſt, the ninth reſurrection; and, although moiſtened again, they revive no more. Part of their rapidity and activity is loſt in each reſurrection, ſo that the laſt is but a ſimple change from immobility to languid contorſions of the members.

Here then are three ſpecies of animals, inhabiting the ſand of roofs, which nature has permitted to revive after death. Theſe three are the only inhabitants of this ſand, at leaſt I do not think I have ever ſeen other animated beings there, having

ing a permanent abode. They are not the only animals, however, that enjoy the privilege of reſurrection; others alſo poſſeſs it: among theſe, the celebrated eels of blighted corn deſerve to be particularly mentioned. All the world knows that Mr Needham is the author of this famous diſcovery. Examining the internal ſurface of blighted corn, he ſaw with agreeable ſurpriſe that it was compoſed of minute eels, which, on being wet, acquired motion, and gave certain indications of life. Their immediate reſurrection, as he has obſerved, takes place when the ears are gathered ſtill freſh and humid: if they have been gathered ſome time, and have loſt their humidity, maceration is neceſſary; nor will this always be ſufficient for reſurrection; it is even requiſite that the eels remain a given time in the water. When allowed to dry, they become motionleſs, and recover life on humectation. But what chiefly ſurpriſed the author of the diſcovery was, after having preſerved the blighted corn for two years and more, the ſame phenomena were obſerved anew when it was wet.

The fact was too wonderful for others not to endeavour to aſcertain it. It has been corroborated by ſeveral good obſervers, ſuch as the illuſtrious tranſlator of Mr Needham's work, where this diſcovery is ſpoken of (1); by the Count Ginnani

(1) Nouvelles Obſervations Microcopiques.

Ginnani (1); but by Baker in particular, in his excellent treatiſe on the *eels of blighted corn* (2). Among other things, he has ſeen the reſurrection of eels taken from grain that had been dry four years. This obſervation he made before Mr Folkes, then Preſident of the Royal Society, and other friends. But he witneſſed a reſurrection much more wonderful, which was effected after a far longer time. In 1771, he had ſome blighted corn which he had got from Mr Needham in 1744. In his experiments, reſurrection ſucceeded perfectly at the end of twenty-ſeven years (3).

In ſhort, there is not at this day any profeſſor, any amateur of natural hiſtory, particularly in Italy, who does not take pleaſure in amuſing himſelf, and gratifying the curioſity of his learned friends, with theſe admirable reſurrections. For this reaſon, I judge it needleſs to ſtop and prove their reality by new facts, and to ſpeak of the origin and generation of the eels; for we know that this, which is a moſt eſſential part of their hiſtory, has been amply elucidated by the learned labours of Italians. The reſults of ſome of my trivial obſervations only ſhall be related, which will

(1) Delle Malattie del grano in erba.

(2) Employment for the Microſcope.

(3) Journal de Rozier.

will both ſerve as proofs of their hiſtory, and are analogous to thoſe we have given of other reſurgent animalcula.

The external colour of a grain of blighted corn, that has been kept ſome time, is like ſoot: if broken, the internal ſubſtance conſiſts of a dry whitiſh matter, which, examined with the microſcope, changes to a maſs of long eel-ſhaped corpuſcula. They are not only exceſſively dry, but lifeleſs, and ſo confuſed and confounded together, that it is extremely difficult to ſeparate them without rupture.

If the grain has been ſome hours infuſed in water, and the extremity adroitly cut off, without injuring the interior, and then preſſed with pincers, a parcel of minute eels are ſeen paſſing through the hole, juſt like a bit of paſte drawn into a thread. When dropped into water, they ſcatter; and, falling to the bottom, are extended as ſo many ſtraight lines, or a little curved, and remain in this poſition until revival.

Such variety has occurred in the time for revival, computing from the moment of humectation, that I have never ſeen the ſame thing twice. The anguillæ of ſome grains were reanimated in three hours or leſs; and others, in four or five. Some required twenty hours or more; and ſome, complete days. All thoſe of the ſame grain were not reanimated at once; ſometimes

two

two days intervened between the animation of the firſt and laſt. The whole do not revive: ſome are disfigured and lacerated, part are always ſo; but ſome apparently entire and unhurt remain motionleſs. Reſurrection is affected by the ſtate of the weather: it is accelerated by heat, and retarded by cold; but here alſo are irregularities.

It may be uſeful to deſcribe the ſymptoms which announce the revival of the eels. The firſt indication of returning life is a deviation from the ſtraight lines their dead bodies formed: the head and tail begin to curve, though the reſt of the body continues in a ſtraight line. Sometimes the two extremities do not bend: the body only becomes a little arched in the middle. One will gently oſcillate, while the other does not move: ſometimes they approach each other until a circle is formed by the extremities touching. One extremity will reſt on the other, or glide over it, or both are entwined together: ſometimes the whole body is rolled into a ſpiral in more or fewer, in wider or narrower volutions. Theſe bendings, arcs, oſcillations, circles, twinings, glidings, volutes; theſe contortions are formed and deſtroyed, and repeated at firſt very languidly, then in a manner more lively and perceptible. This ſtrange variety of motions, with others which it is unneceſſary to deſcribe, continues during all the time they live. Whence it appears they have nothing that may properly be called

called progreſſive motion, which conſtitutes a difference between them and the other refurgent animals. They never riſe in the water, nor do they crawl on the ſides of the veſſel; they conſtantly remain at the bottom, appearing like a pellicle or ſpot darker or lighter according as they are more or leſs numerous.

If the water fails, whether by evaporation or otherwiſe, the eels gradually become lifeleſs, and motion ceaſes when there is no more water. The other three kinds of refurgent animals have the prudence to fly the places where the water dries; but the eels continue in the ſame ſpot without attempting to eſcape.

In ſeveral hours, they become very dry, and adhere to the ſubſtances below ſo tenaciouſly, that it is difficult to ſeparate them without breaking: when wet, they ſeparate eaſily, eſpecially with the point of a needle. They ſoon ſoften, and, becoming pliant, it is evident they are of a gelatinous conſiſtence: and an iron inſtrument cannot touch them without injury. This, at leaſt, happens while alive: when dead ſome days, they are ſtill very fragile, yet have more cohesion than one would think: they reſiſt the point of a needle, and do not ſuffer from a drop of water let fall from a conſiderable height. If dry only a quarter of an hour, the contact of water reanimates them; and in a little they become as

vivacious

vivacious as before. Urine, ſalt water, and vinegar, produce ſimilar effects, though fatal in other circumſtances, as we ſhall ſee. When dry during ſome days, they require a full hour for revival. If one has patience to wet them, and allow them to dry, death and reſurrection will be ſeen in an important limitation: which is, the oftener humectation is repeated, the leſs the number of reſurgents will be, and the longer time required for revival. I had a number of lively eels in a watch-glaſs, the firſt time they were revived: one thouſandth part did not revive the eleventh time, and the ſeventeenth there was not one. I have often repeated this important fact, and always with the ſame conſequence, except that the reviving eels either went beyond the ſeventeenth time, or died before attaining it. Not only wheel animals, ſloths, and the minute eels of roofs, but alſo thoſe of blighted corn, enjoy the property of reſurrection circumſcribed within certain limits, beyond which it is loſt. The body to revive muſt be entire. Eels, cut into two or more parts, though often wet, and remaining long in water, never exhibit any ſign of motion. All ſenſation is loſt on diviſion in two, after a ſlight univerſal vibration or convulſion of the body.

I have ſubjected the eels, as well as wheel animals, to different experiments, and firſt to electricity, uſing Franklin's battery. Thoſe alive died

died inſtantaneouſly, and thoſe dead at the time, loſt the property of reſurrection. This did not ſurpriſe me: for almoſt all were broken or diſfigured by the traverſing ſhock. There was a difference in the reſults, if the blighted corn was ſubjected to the ſame experiment: few revived when the grains had been previouſly macerated: if the grains were dry, many recovered life.

As ſalt water, urine, and vinegar are unfit for reviving the anguillæ, at leaſt if they have been only a ſhort time dry, ſo are they fatal to them when revived; not ſo inſtantaneouſly, however, as to other animalcula, for eels will move in them ſome hours after immerſion.

A vacuum does not prejudice their reſurrection, whether the firſt time after proceeding from the grain or in future: only, reſurrection is not ſo ſoon accompliſhed as in the open air.

The heat of the ſun or the fire at 140° kills them in ſeveral hours: motion and life are almoſt immediately deſtroyed at 144° or 149°. Heat is a more powerful agent on wet grains than on dry. The obſerver will commonly have many eels from grains that have ſuffered 138°: moſt part are killed at that degree if the grains are wet.

When freezing water becomes ſolid, the eels ceaſe to move. Cold 8° below 0 does not deſtroy

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ſtroy the reſurgent property, and life returns on melting of the ice.

Thoſe who have never ſeen the eels of blighted corn will find them deſigned, fig. 3. 4. 5. Pl. 4. as they ſwim in the fluid before a magnifier not very powerful. Seven blighted grains are repreſented of their natural ſize and figure, fig. 6. and three magnified, fig. 7.

Plants are beings ſo analogous to animals, that he may be excuſed who has defined them *rooted animals*. In the works of Valliſneri, Buffon, Bonnet, and, laſtly, of the Abbé Corti, may be ſeen the numerous and various traits of analogy between theſe two claſſes of organiſed beings. The ſubject of which we treat preſents a new analogy: for as different animals revive after death, ſo do many plants ſpring again after they have periſhed. It would be departing from my plan was I to ſay as much of them as I have ſaid of animals: and I ſhall be content with mentioning two, the *noſtoc* and *tremella*. The noſtoc, ſo named by Paracelſus, is a terreſtrial plant, whoſe ſudden appearance in places where there was no ſign of it before was conſidered by the ancients rather as a prodigy of heaven or earth than as a plant. Thus they denominated it *Heaven's flower and Earth's flower*. It is ſeen in all ſeaſons, but particularly in ſummer after heavy rains. Though it ſprings in every ſoil, it prefers meadows

meadows, arid lands, and ſandy valleys. The colour is a browniſh green; the figure irregular, and reſembling a leaf careleſsly folded. When ſeparated with the fingers, ſome reſiſtance is felt, ſuch as one feels on tearing a young leaf. If a ſudden drought happens, the noſtoc contracts and dries, remaining only a ſhrivelled fine thin ſkin. If a ſudden and heavy rain falls, it again becomes green, and reſumes its original ſize. Therefore the noſtoc, as Reaumur, who has furniſhed me with this intelligence, obſerves, is a plant of a ſingular kind, ſince it recovers life after being in a ſtate which to others would be permanent death (1).

(1) I have never been able to find the *tremella noſtoc*; but I have made a few experiments on ſome plants of the ſame genus. There is one, of a beautiful yellow, which in damp or wet weather appears on decayed wood. The largeſt, when in full vigour, that I have met with, is about an inch and a quarter long, and about an inch high, of an irregular figure. I believe it is the *tremella dilequeſcens*. When allowed to dry, it becomes of a deep brown colour, not an eighth of an inch in ſize, and very hard. By wetting it, the full ſize and figure are re-acquired. It may be repeatedly dried and moiſtened without injury. I think it is longer of expanding and of the expanſion being complete according to the period of deſiccation and the number

The ſame privilege is poſſeſſed by the tremella, which is an aquatic plant, placed by botaniſts in the claſs Conferva (1). If it chance to be in a veſſel where the water fails, it dries and loſes its verdure; but water being ſupplied, it ſoon recovers its original ſtate. Nature does the ſame as art. I have ſeen, from the beginning of July till

ber of revivals; but this fact merits farther inveſtigation. After eighty days deſiccation, it immediately revived. —T.

(1) Naturaliſts have lately diſputed much concerning the nature of Confervæ. By chemical analyſis, they give products chiefly peculiar to the vegetable kingdom: 1. They give out an acid, and a ſmall quantity of ammoniac, combined with pyromucous acid; 2. They contain muriat and carbonat of potaſh; and, although ſoda may be found in them, we know that it is alſo found in many vegetables; 3. The quantity of aſhes is rather an indication of their vegetable nature. This analyſis was made by Vauquelin, who concluded from it that confervæ had a greater relation to the vegetable than the animal kingdom. M. Decandolle has compared the ſentiments and diſcoveries of modern naturaliſts on the ſubject; and, in a judicious memoir, he thinks 'Confervæ are neither intermediate beings between animals and vegetables, polypi, nor *polypiers*; neither are they aggregates of animalcula, but real vegetables, analogous to tremellæ, fuci, and lichens,' *Rapport ſur les Conferves*, Journal de Phyſique, tom. 54, p. 421.

till the end of October, a ditch for watering land covered fifty times with the beautiful verdure of the tremella, and ſeen it as often diſappear, when there was no water. Colourleſs hairs or wool only were viſible at the ſides and the bottom, which, the microſcope ſhewed me, conſiſted of the tremella dry and dead.

What can be the reaſon why theſe animals and plants are thus privileged, in compariſon to many others which, periſhing once, periſh for ever?—Shall we perhaps aſcribe it to the ſimplicity of their ſtructure? But this opinion or conjecture does not ſeem well founded. There are many animals that never revive, whoſe ſtructure is as ſimple, or even more ſo, than that of the reſurgent animals. Are not many infuſion animalcula, which are compoſed of a ſimple aggregate of veſicles, undoubtedly leſs complex than wheel animals, which are provided with veſſels, wheels, inteſtines, and ovaries? Yet they do not recover life when once it is loſt. Simplicity of ſtructure would even ſeem an obſtacle to their reſurrection; for the ſimple membrane of ſeveral ſpecies burſts on evaporation of the water: the animal is diſperſed, and reduced to an unconnected and diſordered heap of fragments.

The arm-polypus is no leſs ſimple than the animalcula of infuſions, being compoſed but of a granulated gelatinous ſkin. If ſimplicity of ſtruc-

ture influenced the reſurrection of animals, the arm-polypus would certainly be one; and it ſeems ſo much the better adapted for reſurrection, as it continues alive notwithſtanding every method has been taken to deſtroy animation. It is demonſtrated, that theſe polypi ſuſtain no injury by being turned ſeveral times outſide in, like a glove, or by being cut aſunder. If the head is cut off, a ſort of hydra with many heads ariſes, each of which receives food by a different mouth. If theſe new heads are cut off, new hydras ſpring up; and each head creates a polypus fit for the formation of more hydras. In ſhort, every particle, even the ſmalleſt fragment of a polypus, unfolds and becomes a new polypus. If an animal ſo mangled and lacerated does not die, will it not be very credible that, only being allowed to remain dry, it may ſtill retain the faculty of reſurrection? But facts prove the reverſe. The arm-polypus always dies when the water evaporates; and this happens equally whether it is immediately expoſed to the air, or lies concealed among its native ſub-aquatic herbs. When the water is almoſt exhauſted, the arms are retracted into the animal; it contracts within itſelf, and dies. It never recovers, though water is copiouſly ſupplied. I ſpeak of the arm-polypus, for it is the only ſpecies I have been able to find, and is the ſmalleſt of Trembley's arm-polypi.

After

Next to polypi and infuſion animalcula, according to my deſcription, the organization of the ſloth ſeems to be the moſt ſimple. We may ſay the ſame of the anguillæ of tiles and blighted corn, two ſpecies of ſerpentuli, which may properly be claſſed with ſo great a number of the inhabitants of fluids from their organization. Under the tremella in water are often found minute eels very like thoſe of tiles in ſize, ſhape, and ſimplicity of organization. I have frequently had the curioſity to let them dry, by the water evaporating: all endeavoured to conceal themſelves where the filaments of the tremella were thickeſt; and, when evaporation was complete, they periſhed, remaining partly entwined among the filaments, and partly heaped above one another. If immediately wet, they revive; but never, if a few minutes elapſe.

The eels of vinegar give the ſtrongeſt evidence of vigour. Though they continue motionleſs when the fluid fails, and are apparently dead, they recover life and action, if wet, after a quarter of an hour. Sometimes I have ſucceeded in reviving them after half an hour. I do not call this reſurrection: if it was ſuch, I cannot ſee why it ſhould not ſucceed anew when wet with vinegar, in even a longer time. We may rather ſay, they do not die ſo ſoon as the eels of the tremella, and many other inſects left dry:

life, though ſuſpended, is preſerved, and appears on humectation.

I can diſcover no greater ſimplicity in the tremella and noſtoc than in many plants that do not revive. Let us throw a haſty glance on the truffle. What vegetable is more ſimple? No roots, tendrils, or fibres, internal or external; a ſubſtance equally compact and uniform throughout, only interrupted by veins ſimilar to thoſe winding on ſome ſpecies of marble. It has no analogous organization with other plants, either terreſtrial or aquatic; yet truffles, after once drying in the air, do not revive if put in water.

Theſe united facts prove the fallacy of thoſe opinions which attribute the reſurrection of animals to the ſimplicity of their organization. But to what other principle can we recur? for we are here conſtrained to proceed on conjecture rather than evidence and the view of truth. I ſhall ſuggeſt an hypotheſis without engaging to ſupport it. Haller's experiments demonſtrate, that the vital principle of animals with a heart originally reſides in the irritability of this muſcle. His experiments are too well known to need repetition. In animals which have no heart, it is more than probable that the principle of life reſides in the irritability of their muſcles. This being admitted, if the ſtate of the animals is ſuch that the irritable nature of the heart and muſcles is deſtroyed,

ſtroyed, ſo as to leave no hope of reparation, it is clear that the animal not only dies, but muſt always remain dead: if the irritability is ſuch that it may be re-excited, either naturally or by art, it is indubitable that the animal will paſs from death to life. It will not ſignify though it remains dead a long time, even for an age. The reader comprehends my idea. When wheel animals, ſloths, and the eels of tiles are deprived of water, their irritability is evidently loſt, and they die. Other animals, having once loſt this irritability, never recover it; but it is awakened in wheel animals, ſloths, and eels, and they return to their original life by humectation.

From the ſame principle, may we explain why in certain caſes theſe animals loſe the reſurgent property when expoſed to powerful heat or penetrating odours, or when ſome liquids and electricity act upon them. Such agents injure the muſcular ſtructure, as appears by the rupture of the body and deſtruction of the irritable power reſiding in it. This perhaps is the reaſon why frequent humectations prejudice reſuſcitant animals; for I have really ſeen it; and in particular obſerved, that the members of the eels of blighted corn were injured and lacerated by repeated humectation.

We muſt conclude from the whole, that as irritability reſides in the glutinous part of the

muſcle, this part of reſurgent animals has qualities very different from the irritable parts of other animals, though we are profoundly ignorant of what conſtitutes the difference, becauſe we are profoundly ignorant of what the gluten conſiſts.

I wiſh to be ſincere. A concluſion againſt the hypotheſis may be deduced from my experiments. Irritability is recogniſed by its appearance, that is, on touching the muſcular fibre with any ſtimulant, it contracts and becomes rigid. I have often ſtimulated the muſcular ſubſtance of the eels of blighted corn and tiles, with an extremely fine iron point, and attentively obſerved the conſequence. The muſcular fibre always ſeemed to contract a little when touched, but I muſt acknowledge the ſame thing happened to the anguillæ of vinegar, and to other analogous animalcula, which do not enjoy the privilege of reſurrection. There are even ſome aquatic and terreſtrial vermiculi more irritable than the eels, ſince with the moſt gentle touch they contract and ſwell, until they become many times as thick and ſhort. The objection is therefore confined to this: there are ſome animalcula which do not revive, though as irritable, even more ſo than thoſe that do. But it does not affect my hypotheſis, for the principle of reſurrection is not placed in the greateſt and moſt perfect irritability,

ty, but in an irritability which, after ceſſation, may be renewed by means of certain circumſtances, though it otherwiſe appears to be leſs active than in other animals.

If this hypotheſis does not ſeem fully applicable to plants, in what concerns their irritability, ſince we know only a very ſmall number poſſeſſing that property, ſtill it may be applied to what reſpects their organization. Dried plants in general do not recover life, probably becauſe they are ſo much injured in drying as to become incapable of imbibing the juices provided, and converting them into their own ſubſtance. Thence do they periſh, and are totally deſtroyed. If ſuch diſorder is not occaſioned by drying, and the organic action of plants revives when they are ſoftened, and reſume their original form, it is undoubted that they will then recover their priſtine verdure and natural freſhneſs. This may be the phyſical cauſe why the tremella, noſtoc, and ſome other vegetables revive.

This tract may be terminated with ſome reflections on thoſe beings which we can kill and bring to life at pleaſure. When preſented to the mind we are aſtoniſhed, becauſe they are iſolated beings: they form a ſeparate claſs, and the ideas they ſuggeſt are adverſe to thoſe received of the animated world. But when it is proved by a ſeries of innumerable facts, that all is gradated in nature,

nature, that these beings are connected with other beings, consequently that the isolations exist only in the general system, wonder should cease, or at least be diminished, since it only arises from our ignorance of the relations that connect the classes enjoying the privilege of resurrection with those that do not. This is not the only isolated fact which has existed, and at first been considered an exception to general laws. The works of Reaumur, Trembley, Bonnet, will communicate many. The exceptions appeared singular, because they were seen but in one instance. A plant, an animal of a new genus, or possessing peculiar properties, is the origin of such exceptions, till farther observation and solid experiment proves them adapted to several cases, either in the same circumstances, or under modifications, and proportioned as human industry has been applied to diversify the number of subjects, and render them so numerous, that they can no longer be called exceptions. The surprise that affects the mind from something new or extraordinary gradually diminishes and vanishes entirely. One or two examples will corroborate and elucidate these reflections on resurgent animals.

One of the most effectual methods to destroy animals is to cut them in pieces: nothing is more common or better known. To say this is a mode of

of multiplying ſome ſpecies is affirming what has a fabulous appearance. Yet it is certainly the caſe with the arm-polypus; and ſhall we admit that the diſcovery is bounded by this animal? The ſcalpel need only be applied to others, to prove that the fact is wonderfully extenſive. Thence are the reproductions of the earth worm, the boat worm, the freſh water worm, ſome leeches, ſea ſtars, and nettles. While art effects prodigies on theſe ſpecies, nature prepares ſimilar ones in ſilence. I ſpeak of propagation by the natural diviſion of the dart millepede, many races of club, funnel, and bell polypi, and infinite infuſion animalcula. It is alſo found that nature ſometimes forms not only two from a ſingle animal, but even four, nay, a multitude ſo prodigious, that as many ariſe as there were atoms in the generating animal. The polypus does not terminate the wonderful progreſs of diſcovery. It is a chain paſſing from vegetables to animals, and leads to man. The tremella is the link connecting animals and vegetables; it is a real zoophyte. It has been diſcovered that the filaments of which it is compoſed divide ſpontaneouſly, and that a complete plant ſprings from each diviſion (1). The polypus is joined to the tremella,

(1) *Adanſon*, *Fontana*, *Corti*, *Deſauſſure*, and other philoſophers, have diſcovered that certain ſpecies of tremella have

tremella, and is united to many races that divide like itſelf, which in a ſimilar manner are linked to other ſpecies. Reproductions are not effected in the ſame way in all. One may reduce a polypus, a freſh water worm, a ſea ſtar, or nettle, to the ſmalleſt fragments, and be certain that each particle will reproduce itſelf. The earth and boat worm muſt not be cut in pieces ſo ſmall when we wiſh to obtain reproduction. If a ſnail is decollated, a new head will germinate, but the ſevered head will not acquire a body. Water newts and frogs, while tadpoles, recover their tails and limbs when deprived of them; but if mutilated in other parts of the body, they periſh (2). No warm blooded animal is yet known which reproduces itſelf when cut in pieces, but animals recover large pieces cut away. Duhamel has ſeen a ring of fleſh cut to the bone reproduced in a chicken. Similar reparations are every day obſerved in the cicatriſed wounds of men and animals: and we have certain evidence of the reparation of the

have a real progreſſive motion. From this ſome naturaliſts ſuppoſe that it is endowed with animality, or conſtitutes a link which connects the animal and vegetable kingdoms, though properly belonging to neither. Other naturaliſts think theſe ſpecies are real vegetables, but that their motion or progreſſion is owing to ſome mechanical means.—T.

(2) Prodromo ſopra le Riproduzioni Animali.

the tibia in a man (1). It is thus that the discovery of the polypus, which at firſt ſeemed to revolt againſt rules eſteemed general, has extended to ſo many links in the animal chain. But has this ſucceſs ſo great, this advance ſo rapid, exhauſted the ſubject of reproduction? No, aſſuredly: it will appear but little known, if we conſider how limited the number of animals on which our experiments have been made, compared with thoſe on which we may make them. The element water is moſt favourable to reproductions. How many inſects, worms, reptiles, zoophytes inhabit the briny waters of the ocean, the freſh water of rivers, pools, marſhes, and ditches, which were never ſubjected to experiment, and which, from their great ſimilarity, whether in the modes of life and propagation, or in ſhape and internal configuration with reproducing animals, are doubtleſs calculated to reproduce their parts?

Hermaphrodiſm, until the beginning of this age, has been conſidered more chimerical than true. Nature ſeemed to declare againſt it. Yet in how many hundred animals has it not been found by the diligence of modern naturaliſts? This admirable property paſſes by degrees from one ſpecies to another. The polypus is a perfect hermaphrodite

(1) The celebrated Bernard Moſcati, ſurgeon in Milan, who was an eye witneſs, told me this fact.

hermaphrodite without ſex: it multiplies both by diviſion and ſhoots. The puceron of plants is leſs an hermaphrodite; it has a ſexual diſtinction; and although it propagates without copulation during ſummer, at the end of autumn it is obſerved to copulate. Earth worms, ſhell ſnails, naked ſnails and many ſpecies of ſhell fiſh are leſs hermaphrodites: each is at the ſame time male and female, but inſufficient alone for generation. They give and receive; they fertilize and are fecundated.

The diſcovery of reſurgent animals is far from being as extenſive as that of reproduction or hermaphrodiſm; but this ariſes leſs from the ſcarcity of ſuch beings than the rareneſs of the philoſophers who have entered on this branch of natural philoſophy. Leeuwenhoeck was the firſt who, by his reviving wheel animal, drew the curtain from before theſe objects: and ſeems ſurpriſed at a fact which was unique and unexampled in nature: and, indeed, no one thought there was another creature in all the animated world poſſeſſed of this prodigy. But ſince the profound reſearch of more modern naturaliſts has diſcovered others, I doubt not that the number of theſe wonderful beings will increaſe as the ſtudy is cultivated.

The ſame gradations perceptible in hermaphrodites and in reproducing animals cannot be expected in the reſuſcitant. There may be a greater or leſs

leſs degree of reproduction: an animal may reproduce more or fewer organs: and an animal may be more or leſs an hermaphrodite, if hermaphrodiſm is taken in a comprehenſive ſenſe. Both theſe caſes are poſſible, and, as we have ſeen, are realiſed by facts. But we cannot reaſon thus on the animals reviving after death; we cannot ſay an animal dies more or leſs, or that its reſurrection is more or leſs. Death and reſurrection are two indiviſible acts, ſo much, that neither has any gradation in the animals hitherto known; and it cannot be expected in thoſe we ſhall hereafter diſcover. It is not that theſe animals have no particular gradations among themſelves, or are not connected by them with others of the ſame kind: nor are they difficult to be obſerved. We have ſeen the ſtate to which ſome animals are reduced by cold: it cannot properly be called death, for there is a thread of life, a leſſer life, which may connect them with reſurgent animals. The verdure of moſt plants is loſt in winter: they have little ſap, and it is motionleſs: they ceaſe to imbibe nutriment, to grow, to multiply: thence are they in perfect inaction. Beſides inſects, how many animals do we ſee with this leaſt degree of life, not excepting ſome warm blooded, and among them birds? I am far from coinciding with Guaguin in this deſcription of Muſcovy: 'Populos quoſdam in Lucomonia Regione

'Ruſſiæ

'Ruſſiæ habitantes quotannis vigeſima ſeptima No-
'vembris die, ut ſolent hirundines, et rana, ſic et
'ipſos præ frigoris hyemalis magnitudine mori:
'poſtea redeunte vere vigeſima quarta Aprilis die
'denuo reviviſcere.' At the ſame time, we cannot deny that man himſelf may ſometimes be in a ſimilar ſituation with animals overcome by cold, as when he has been immerged in water, without entirely ceaſing to live. I will not affirm with ſome philoſophers, becauſe there is neither pulſation nor reſpiration evident in the body, that pulſation of the heart and circulation of the fluids are ſuſpended; but I would rather think with Haller, that theſe motions are only too faint and obſcure to be externally perceptible. Examples of this we have in ſome animals half drowned, where a degree of motion is always ſenſible in the heart and in the blood. The life of man and animals half drowned cannot be more feeble; and we may regard it as another point in the paſſage from the reſuſcitant to thoſe animals which do not revive.

There are other two ſtates very ſimilar to the death of reſurgent animals. One is the ſtate of the embryo in a fecundated egg before it has experienced the heat neceſſary for expanſion: life is incomplete: there are but the rudiments of life. The other is the ſtate of a chryſalis among inſects. When the caterpillar has loſt its natural form,

form, it assumes that of a shapeless mass, without the vestige of feet or wings: it ceases to feed; indeed to eat would be impossible, for the organs are wanting: it has no longer loco-motion; and we should really believe it dead, but for some inflection and contortion of which the posterior part is susceptible. Apparent death is still more sensible in the nymphs of many worms; no stimulus can awaken any symptom of life or sensation.

Thus there are situations in nature somewhat similar to the state of dry resurgent animals: and these situations may be protracted or abridged at pleasure the same as with resurgent animals. That torpid animals may never awaken from their lethargy; that the embryo may never expand in the egg; that flies and butterflies may never proceed from the nymphs and chrysalids, nothing is requisite but to keep them continually exposed to cold. The reverse will happen on exposure to heat. Probably there are gradations of connection more immediate and more direct between the animals that revive and those that do not. Life, however feeble and obscure, is always life: between it and death there is a distance as great as between existence and non-entity. An animal, whose life might be suspended from an impediment to the mutual action of the solids and the fluids, would be the link connecting the least

degree of life with death. Such an animal is yet unknown: but we ſhould not deſpair of finding it, ſince ſo many are diſcovered which connect and more intimately unite the animated chain. Let us only reflect, that natural hiſtory is yet in its infancy, and that our diſcoveries are nothing, compared with what we have yet to diſcover.

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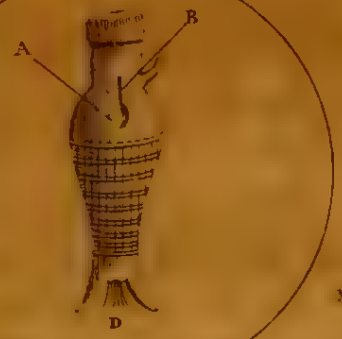

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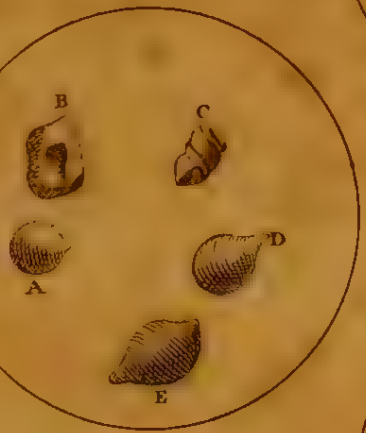

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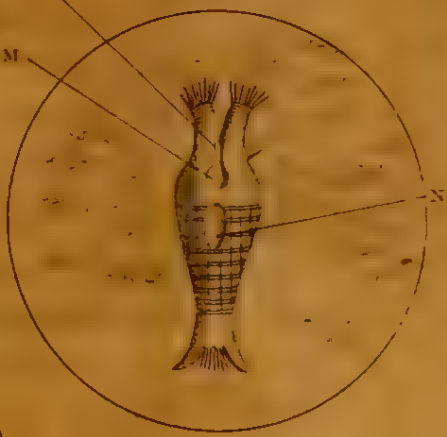

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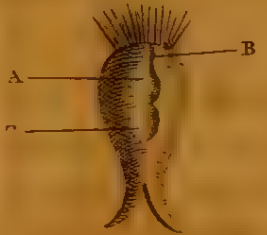

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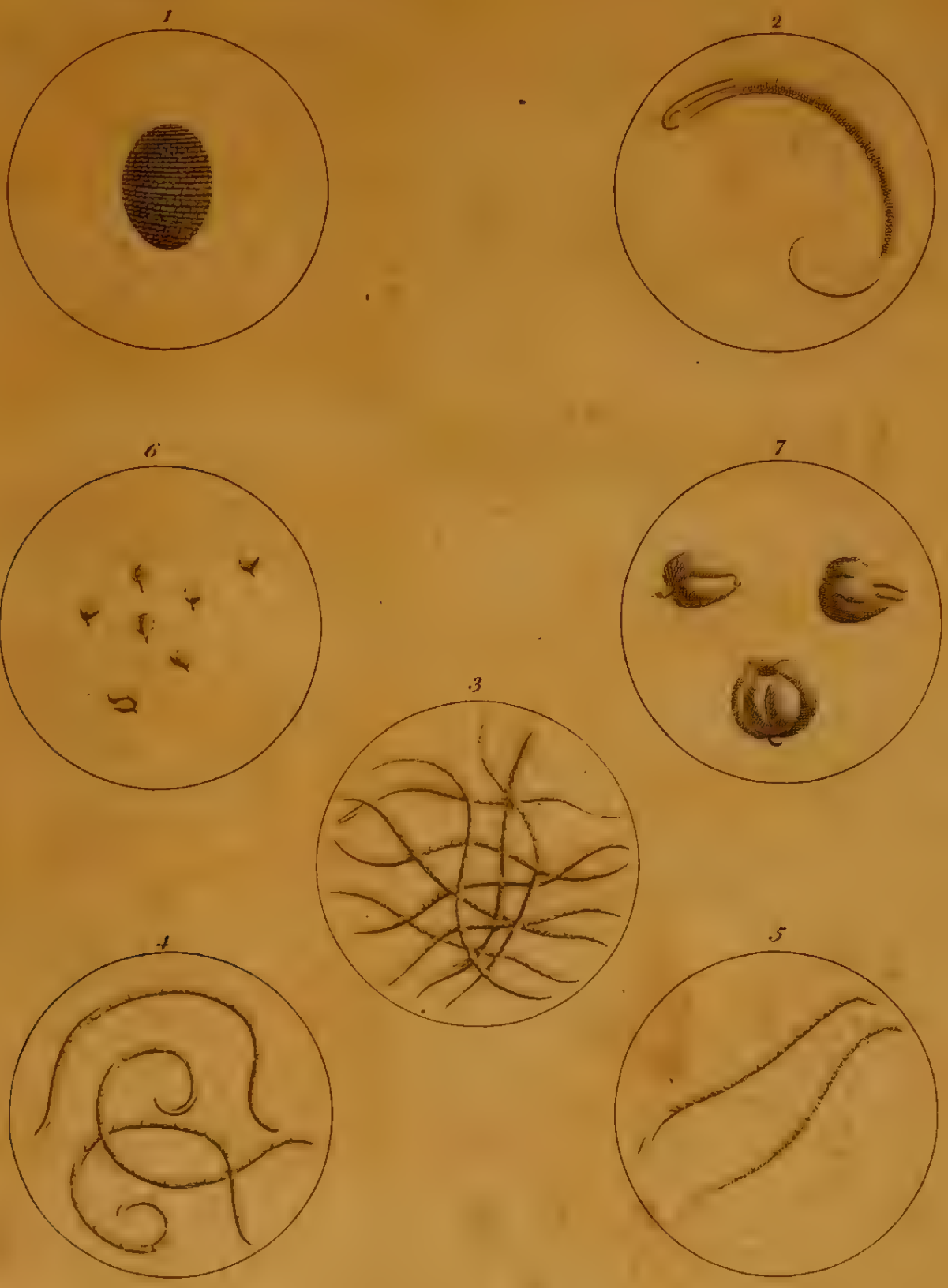

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OBSERVATIONS AND EXPERIMENTS

ON

THE ORIGIN OF THE PLANTULÆ OF MOULD.

THE mould, which I have examined, and intend ſimply and briefly to deſcribe, ſprings on moiſtened bread, apples, pears, melons, or gourds, beginning to ſpoil. It may be divided into two kinds, the one very ſimple and eaſy to be obſerved: the other complex and intricate, which can only be underſtood by a generic deſcription. Let us begin with the former.

One ſpecies is without branches, and each filament bears a globule at the ſummit, pl. 5. fig. 3. another is ramoſe, but with this difference, that ſome plants have a globule at the vertex, while others have none, fig. 2. Theſe globules ſhall always be termed the minute heads of mould;

but with reſpect to them there is a remark neceſſary. Without the microſcope, we ſhould ſuppoſe them perfectly ſpherical, and even with it they appear ſo if viewed from above; but when examined below, that is where the ſtalk is inſerted into the head, we obſerve that all, or the greater number, are ſhaped like muſhrooms, or, to ſpeak more philoſophically, they are real muſhrooms. Two filaments, with globules, are repreſented, fig. 7. and highly magnified, fig. 8. A kind of mould ſometimes grows on pears, which is actually a ſhrub in miniature, univerſally adorned with ſpherical heads and muſhrooms, fig. 6.

Ramous mould is often attached to vegetable ſubſtances without the aid of roots; but mould wanting branches almoſt always has roots originating from a round corpuſcle, from which the filaments or ſtalks of mould ariſe. It is ſingular, that in proportion as every root gives origin to a greater number of ſtalks, ſo are the filaments it ſends forth below more numerous in proportion. A degree of reſiſtance is felt on tearing mould from the ſubſtance where it ſprings, which is the conſequence of the roots being well fixed. When torn up, they appear very crooked, while the ſtalks, that have not ſuffered from the impreſſions of the air, are perfectly ſtraight. Many of theſe are of an equal thickneſs throughout, and become a little ſmaller towards

wards the top. The deſcription will, perhaps, be better underſtood by fig. 3.

Mould is at firſt of a moſt beautiful white; it next acquires a yellowiſh tint, and at laſt blackens; however, the heads grow much blacker than the ſtalks and branches. The origin and increaſe of mould is nearly in proportion to the heat of the atmoſphere; its appearance and vegetation are never more ſudden than during the heats of ſummer. A ſubſtance which does not exhibit a ſingle ſtalk of mould at night, will often be covered in the morning by mould, which has then attained its full ſize and maturity (1). It does not increaſe ſo much in height as in thickneſs, and the heads, already black, are always of larger ſize than when they were young and white.

Mould never riſes ſo beautiful and vigorous as when vegetating under ſome veſſel or receiver, providing the communication with the external and internal air remains. The reaſon is evident.

(1) Bradley remarks that the ſeed of mould has vegetated in three hours, and in about ſix more, the plant had attained its perfect ſize, *Philoſophical Tranſactions*, 1729, p. 491. However, ſome mould takes much longer time. Nay, it is often very long before mould is produced on a ſubſtance, either from abſence of the ſeeds, or the ſubſtance not being well adapted for its vegetation.—T.

The ſtalks being very fine near the ſummit, and bearing on the vertex a round corpuſculum, which oſcillates the filament by its own weight, as the ear of corn occaſions oſcillation of the ſtalk, we may eaſily perceive that every breath of air, however gentle, will bend, break, and deſtroy their moſt delicate texture, which does not enſue when the moulding ſubſtances are put under a receiver. Beſides, their humidity is better preſerved, a condition moſt eſſential for the production and increaſe of mould. In the courſe of theſe obſervations, I have always uſed receivers. The prejudicial conſequences of agitated air are viſible, fig. 1. which repreſents two ſpots of mould with heads, viewed with the naked eye when taken from under the receiver, and expoſed a ſhort time to the action of the air: their natural direction is loſt, and the ſtalks bend in various directions.

Some ſubſtances, put in a ſituation to acquire mould, gradually diſſolve into a kind of acid fluid, wetting the circumjacent parts; it is preciſely here that the mould hitherto mentioned ſprings. A ſmaller portion of moiſture likewiſe exhales from the ſame ſubſtance, which adheres to the inſide of the receiver, in the form of a pellucid aqueous veil, and increaſes ſo conſiderably as to form large drops that run down the ſides of the receiver in winding ſtreams: an equal quantity

tity of mould grows on theſe ſtreams, as may be obſerved if the glaſs receivers are tranſparent.

But the other kind of mould, which in the beginning we have ſaid is very complicated, always ſprings on the immediate ſubſtance of the vegetable itſelf, and particularly prefers gourds and moiſtened bread. When theſe begin to acidify and corrupt, a thick white covering of mould appears on the ſurface, which in a few hours is an inch high, and, when ripe, three inches or more. This is different from the laſt ſpecies, which, at complete maturity, is ſcarcely half an inch high. We have already obſerved, that each plant may be examined ſeparately, and without confuſion, and that the examination may be diſtinctly continued until maturity; but it is otherwiſe with the mould before us; for it is impoſſible to examine each plant diſtinct and ſeparate. The immenſe number of them when the mould begins to ſpring, the interlacing of the ſtalks and branches which entangle and interweave in a hundred different ways, is a complete obſtacle to it: the eye is abſolutely confounded, viſion is confuſed, and can only diſtinguiſh an intricate aſſemblage of ſlender filaments, which, as vegetation advances, become more and more deranged, therefore a general deſcription of the whole together muſt be given. The ſtalks, torn from the ſubſtance where they ſpring, ſeem to have no

roots. Numerous twigs and branches proceed from each, which are frequently of as great diameter as the parent ſtalks whence they originate.

While the ſtalks ſucceſſively vegetate and extend, ſeveral groups of ſmaller branches ſpring out laterally, with minute heads at the vertex. Theſe are partly fungi-form, partly globular, and, as the ſtalks riſe, they blacken and come to maturity. Additional new ſtalks next appear; and multiplication continues ſo long as the mould vegetates. This little foreſt of filaments is terminated by moſt minute points, and wholly covered with black globules.

Thus does mould originate and arrive at maturity. We need not inquire whether it is a real vegetable: it evidently is ſuch, by the obſervations I relate. But theſe vegetables, or microſcopic plants, do not poſſeſs two properties common to other plants. Ligneous and herbaceous plants, expoſed to natural light, always tend to take a direction perpendicular to the horizon. They endeavour to attain it when an impediment intervenes. The experiments of M. Bonnet are excellent, as may be ſeen in his work *Sur l'uſage des Feuilles*. But we do not ſee this tendency to perpendicularity in mould; for, although many ſtalks are perpendicular, it is not eſſential to their nature, and innumerable others are in a different direction. If a plant grows in the rent of a wall, though it firſt

appears

appears in a horizontal poſition, it ſoon riſes perpendicularly, and ſo continues to grow. It is otherwiſe with mould. I have often cut a piece of melon, gourd, or bread, into a cube: mould vegetated on the four lateral ſurfaces, and the ſtalks had conſtantly every other than a perpendicular direction (1).

The other property, which was diſcovered by the celebrated Geneveſe naturaliſt, is the tendency of plants to turn towards the light. In addition to the facts he recites, which are ſufficient to aſcertain the property, I have frequently obſerved it in legumes growing in infuſions, ſhut up in a preſs: the plants always bent towards a chink that admitted a ſlender ray of light; and if this chink was ſtopped up, and a new one opened in a different part of the preſs, the plants abandoned their original direction for this new one. I could never diſcover that light had the leaſt influence on mould.

If ripe mould is ſhaken, a kind of black duſt falls from it, which the celebrated botaniſt Micheli has ſuppoſed the ſeed of the plantula; but the

(1) This may generally happen; but I have, in many inſtances, ſeen an evident tendency to perpendicularity, and vegetation in a ſtraight line. Deviation from it may perhaps be chiefly occaſioned by the extreme delicacy of the ſtalk.—T.

the elder Dr Monti, alſo a very eminent botaniſt, has called the truth of his opinion in queſtion, and rather inclines to think that mould ſprings by ſpontaneous generation. Before diſcuſſing this matter, which is ſo intereſting, it is proper to examine where the duſt is found, which renders it neceſſary to make a brief analyſis of the heads of mould, which can only be done by ſeeing it ripen. Before maturity, the heads are of a whitiſh and yellowiſh colour, the ſurface very ſmooth, and they are firmly attached to the ſtalk. Broken with a fine needle, they ſeem membranaceous, and full of a granulated ſubſtance. If, inſtead of being broke, they burſt, a number of moſt minute round ſeeds ſometimes come out: theſe are found both in the ſpherical and fungiform heads. When they blacken, the appearance changes; the ſurface ſeems unequal; it is lacerated in ſeveral places, and reſembles a parcel of black rags. Many ſeeds are ſeen when opened; but young mould has white ſeed, and old or ripe mould has black. By letting a drop of water fall on the heads, the ſeeds are ſeen more diſtinctly and in greater abundance. On contact with the fluid, or a little afterwards, the heads burſt and ſcatter a cloud of ſeeds around; ſo that I may affirm, without danger of exaggeration, a thouſand are in each head. The unripe heads do not open in this manner when wet; they remain

main entire; and it must be remarked, that the ripe ones are not altogether decomposed. Both in the round and fungiform is a little head in the centre, which continues adhering to the stalk; it is cinder-coloured, and does not appear black like the exterior. It is difficult to be detached from the stalk, but, with gentle pressure, a small jet of seeds, resembling those I have described, is raised; after which, the central head becomes a dry, empty skin.

If the heads, black and ripe, are opened by means of water, quantities of seed so great escape, that they adhere to the plants, and the stalks particularly, in such a manner that one would suppose the exterior composed of seeds alone, were they not previously seen in a different state. The deceitful appearance of two plants in this state is represented fig. 9. One is completely covered with seed; the head is magnified, and great part of it also covered. Three stalks are represented fig. 4. The whole seeds of one head are exposed; another head is partly covered by the integument; and the third wholly so.

A quantity of this dust constitutes the powder which blackens the hands when mould is touched; and it is considered real seed, by the celebrated Florentine botanist. To ascertain the truth, he had recourse to a method apparently decisive, which was sowing the dust. He strewed

ed it on ſome vegetable ſubſtances, and ſaw them covered with mould. But the Bologneſe Profeſſor repeated Micheli's experiment without finding it ſo concluſive; the vegetable ſubſtances being equally covered, though no duſt was put upon them. Thus the queſtion was undecided, for I do not know that any other perſon has attempted to ſolve it. Perhaps I ſhall be taxed with preſumption for ſaying, that, by means of experiments analogous to thoſe of Micheli, it has been in my power to aſcertain the fact, but experiments much more numerous, diverſified and connected, which I muſt be permitted to narrate.

Two pieces of moiſtened bread were taken, as ſimilar to each other as poſſible, and from the ſame loaf, ſo as to be perfectly equal; and I endeavoured to attain the ſame equality in all the reſt of my experiments. One piece was ſtrewed with duſt, taken from a quantity of ripe mould heads, in ſuch a manner that the ſurface was faintly blackened: the other was untouched, on purpoſe to compare the production upon each. This was done in ſummer. Next day, the *ſown ſubſtance*, but, for brevity it may be obſerved, that by this is meant the ſubſtance of whatever nature covered with duſt; and by *unſown*, the other vegetable ſubſtance not covered,—on the ſown ſubſtance, I ſay, a ſhade of mould appeared, whereas on the unſown was none. Before the

the third day, both were covered with mould; but that on the ſown ſubſtance was almoſt double the height and thickneſs of the mould on the other. Both ſpecies were the ſame, and perfectly ſimilar to what had produced the duſt. On the fourth day, the mould of the unſown ſubſtance, though not ſo thick, was equal to the other in height; it was even higher the following day, but afterwards continued to become thinner. Theſe experiments were repeated eleven times on moiſtened bread: twice the mould became equally high and thick on both ſubſtances, and nine times it was higher on the unſown one, but thinner. It conſtantly ſprung firſt on the ſown ſubſtance.

Having collected a great quantity of ripe duſt, I thought of varying the portions ſcattered on moiſtened bread. The conſequences were new: When the quantity of duſt was very ſmall, there was almoſt no difference in the height and thickneſs of the mould on either ſubſtance ſown or unſown; however, the thickneſs increaſed by ſowing a greater quantity, and it was never ſo thick as when liberally ſtrewed over the bread: then the mould was a real covering; but in proportion as the thickneſs augmented, the height diminiſhed. The experiments were repeated again and again on apples, pears, and gourds; and

and all the results were, to a certain degree, more or less similar to what are related.

We may, in the first place, deduce, that sowing the dust accelerates the production of mould; secondly, The thickness is increased; thirdly, The height is less. Considering these facts with respect to my object, it seems that the second proves the dust to be the real seed of the mould; for more abundant production arises from scattering it. If the thickness increases in proportion as the quantity sown is augmented, it is natural to suppose the superabundance of mould on sown substances an effect of the dust, or rather of the minute seeds sown; and that all or most part of the mould originates from them. This being the case, we cannot be surprised if mould on sown substances is not so high as upon unsown; for, the plants being more numerous, each can not imbibe the same nutriment from it as may be derived from that which is unsown, where there are fewer. The same also succeeds with other plants, which are smaller and shorter in proportion as they are more crowded together. The first consequence deduced from these facts demonstrates, that the production of sown mould is earlier than that of unsown. I have thought it might be because the substances spoil sooner; since it appears that, by means of the dust, they sooner contract that principle of acidity and putrefaction, on which,

which, as we have already remarked, the origin of mould depends.

The experiments were diverſified. Sometimes I covered a half, ſometimes two thirds, or one, of a ſlice of bread, an apple, a pear, or a gourd, without touching the other half. The half, two thirds, or one third, were juſt in the condition of the ſown ſubſtances. I likewiſe made another experiment. After covering half a ſlice of bread, apple, or gourd, with duſt, the ſurface ſown was applied to another ſurface ſimilar but unſown, and both left in this ſtate ſeveral days. On the whole ſown ſurface a veil of mould appeared, the vegetation of which had ceaſed, becauſe it was ſpoiled by the ſubſtance applied: but no veſtige of mould was ſeen on the unſown part. Thus the hypotheſis is corroborated, that the duſt is the real ſeed of the mould, becauſe that produced on the places ſown was exactly of the ſame ſpecies with what had afforded the duſt.

Notwithſtanding all theſe plauſible and repeated experiments, I was not ſatisfied. Is it not poſſible, ſaid I to myſelf, that this duſt only renders the ſoil more fertile, ſo that it will produce a greater quantity of mould, as the earth fertilized by foreign matter will produce more plants? Certainly it was not impoſſible; and, wiſhing to proceed with philoſophic ſtrictneſs, I judged myſelf obliged to realize or remove the poſſibility: for

for which purpoſe it was proper to cover moulding ſubſtances with duſt taken from different vegetables, different earths, and other matter volatile from extreme minuteneſs. It ſeemed, if the other duſt could contribute to render ſubſtances more fit for producing mould, that it alone did not poſſeſs this property. The roots, ſtalks, and heads of mould, ſtill unripe, were not ſpared. They were dried, and reduced to fine powder, but without effect. For the moſt part, inſtead of the uſual quantity of mould appearing, they deprived the ſown ſubſtances of the power of producing it; and that powder, which did not prevent the production, diminiſhed the quantity of what the unſown ſubſtances uſed to produce. All theſe united facts ſeemed to prove, that the granuli, proceeding from ripe mould heads, are real vegetable ſeeds.

During the courſe of my experiments, I was curious to learn whether the ſeed would germinate when ſown on ſubſtances that naturally did not mould. A quantity was ſown on hard bodies, as glaſs, metals, ſtones, alſo on blot ſheet, writing paper, cotton, ſponge, and the like. All the ſubſtances were kept moiſt: but no traces appeared except ſome filaments, which were perceptible on ſponge. Certain circumſtances are requiſite for the expanſion of the ſeeds, and theſe are found only in particular ſubſtances.

The

The minute ſeeds or duſt of mould poſſeſs the peculiarity of reſiſting a degree of heat, which no other ſeeds can ſupport without loſing the power of germination. After boiling the ſeed in water, I poured the water, then become black, on ſubſtances apt to mould, and where it uſually grows thicker than on ſubſtances unmoiſtened. The ſame was done with duſt expoſed to much greater heat; and I have found, that as this heat does not deprive the ſeeds of the property of reproduction, neither does it alter their ſize or figure, which examination with the microſcope both before and after expoſure has demonſtrated.

But does that mould, which ſprings without being ſown, and by the care of nature alone, on infinite ſubſtances, every where diſperſed, alſo derive its origin from the duſt, which we may ſuppoſe diſſeminated through the air and on terreſtrial matter? If natural and artificial mould are of the ſame ſpecies, and if the artificial is produced by the duſt of the natural mould, I cannot ſee why the latter ſhould not originate from the ſame principle, eſpecially ſince it is demonſtrated that no other part of mould, ſuch as roots and ſtalks, aid the reproduction. The hypotheſis, ſuppoſing that this duſt is inviſibly ſcattered through all, and gives exiſtence to an immenſity of natural mould, is one of the moſt reaſonable hypotheſes in philoſophy. If each ripe head can

furniſh a thouſand ſeeds, and if each ſpot of mould contains a prodigious number of heads, it is clear that in the courſe of years the duſt ſhould be multiplied to exceſs, becauſe, from its extraordinary levity and fineneſs, it may be univerſally diffuſed.

We have certain evidence that ſeeds may be kept a long time without loſing the germinating faculty. My illuſtrious friend, M. Bonnet of Geneva, told me a ſingular fact: In 1748, corn was carried from Sicily to Geneva, and lodged in the magazines of the Republic. Some individuals ſowed part of it in a walled garden, 1771. Notwithſtanding the length of time, it vegetated perfectly and nearly as thick as common to grain of the ſame kind. The wonderful minuteneſs of the ſeed of mould ſeems to adapt it for long preſervation; but I have already given a convincing proof of the fact.

Heat is undoubtedly one of the moſt powerful agents in depriving ſeed of its germinating faculty. In the tract on infuſion animalcula, it has been ſeen that the number of ſeeds which can ſupport the heat of boiling water is very ſmall; and although M. Duhamel's ſingular caſe is cited, where wheat germinated after experiencing 235° in a ſtove, it is here proved that the ſeeds of mould are not deſtroyed by a degree infinitely greater.

greater. Therefore, it is not abſurd to ſuppoſe that ſeeds, which reſiſt the injuries of weather, may preſerve fecundity for ages. Thus we can eaſily comprehend how immenſe the abundance of this vegetable ſhould be, ſince its ſeed multiplies ſo much, is preſerved ſo long, and that it ſhould be ſo copiouſly diſſeminated over all terreſtrial ſubſtances, as continually to be in readineſs for germination, when the requiſites eſſential are preſent.

The firſt of Sig. Monti's doubts concerning the production of mould, which led him to think that it originated by ſpontaneous generation, is reſolved. The other doubt which aroſe from moulding ſubſtances acquiring mould after boiling, the ſame as before it, is equally removed; for if the ſeed does not loſe its germinating faculty from expoſure in a hot chafing diſh, there is no wonder that it is retained at a degree of heat ſo far inferior as that of boiling water.

Although the ſubſtances for my experiments were conſtantly kept under receivers, which was done with the view of obtaining more luxuriant and beautiful mould, communication with the external air was uninterrupted. I wiſhed to diſcover what would ſucceed on cutting off this communication, but previous to that the conſequence of leſſening it. Moulding ſubſtances were put into very large glaſs veſſels: the necks were then drawn to a point by the blow-pipe; and as the point could

be brought to any degree of fineneſs, I had veſſels into which a ſtream of air, no larger than a hair, could be admitted, a little more into ſome, and ſtill more into others. All the incloſed ſubſtances moulded in a certain time. But in thoſe with a very ſmall aperture, vegetation was ſlower; and the mould did not riſe ſo high as when the aperture was larger. The vegetable ſubſtance within always perſpire ſo much, that the vapour collects at the apertures, and obſtructs them, eſpecially if very ſmall, which may be corrected by ſucking out the moiſture; however, if this is neglected, mould will not grow, or hardly at all in very ſmall veſſels.

My curioſity being ſatisfied here, I began the other inquiry, which was the effect of excluding the external air entirely, and this was eaſily accompliſhed by a hermetical ſeal. The veſſels were of different ſizes, ſome might contain ſix pounds of water, ſome only one, and others but a few ounces. This difference affected the mould. In the largeſt, although equally thick as in open veſſels, it never grew ſo high, and was later of appearing: the mould, in thoſe of a middle ſize, was ſhorter, ſtill later, and more rare; its ſtate was worſt in the ſmalleſt veſſels; none appeared in ſome, and in others, a ſlight ſhade was ſcarcely viſible.

Veſſels of three different ſizes, containing vegetable ſubſtances, were hermetically ſealed, and

put

put ſeveral hours in boiling water. In the ſmalleſt was no mould, a little in the middle ſized, and plenty in the largeſt.

To theſe two experiments I added a third, by putting moulding ſubſtances in vacuo; and repeated reſults proved that, during the time the ſubſtances were there, which was always ſeveral days, if by any chance a portion of air inſinuated itſelf into the receiver, a quantity of mould appeared which came to maturity, though very ſhort. None ever germinated when the air was quite exhauſted. Some plantulæ, produced in a receiver where the vacuum was incomplete, are repreſented, Plate 5. fig. 5.

Theſe three different experiments aſcertain that the plantulæ have the ſame relation with the air as other plants, but it is apparently leſs neceſſary to them; for when a thread of air entered the receivers, ſome mould vegetated, yet the leguminous ſeeds within gave no indications of vegetation; neither do ſeeds vegetate in veſſels hermetically ſealed, although vegetables will mould there. The ſimplicity of mould undoubtedly contributes to render the preſence of air leſs eſſential, in the ſame way as animals, which are leſs compound in the ſcale of organization, may be produced, and exiſt in a ſmaller quantity of air than is neceſſary for us.

M. Bonnet, in his judicious reflections on mould, queſtions whether we are ſure that it all belongs to the vegetable kingdom, or whether there may not be ſome ſpecies that approach the mineral, or, at leaſt, are the link of connection between the two kingdoms, vegetable and mineral. This is not impoſſible, when we conſider the amazing diverſity in this claſs of beings, and how little its ſpecies are hitherto known, eſpecially if we conſider that foſſils approach them by occupying the loweſt rank in the order of vegetables.

Beſides the mould deſcribed, I have not neglected to throw a glance on many different ſpecies, and in all I muſt acknowledge, that characteriſtics, ſufficiently deciſive, have been found to judge them real vegetables. But the various kinds obſerved by me are very few, compared with the immenſe number yet remaining to be examined; for there is almoſt no ſubſtance, animal or vegetable, which in certain circumſtances is not liable to mould. Thoſe who attach themſelves to this branch of microſcopic botany will have ſufficient uſeful practice, and perhaps may ſucceed in diſcovering the link connecting vegetables with minerals, which will render philoſophy a moſt important ſervice. For my own part, I ſhall be content if, among other things, I have

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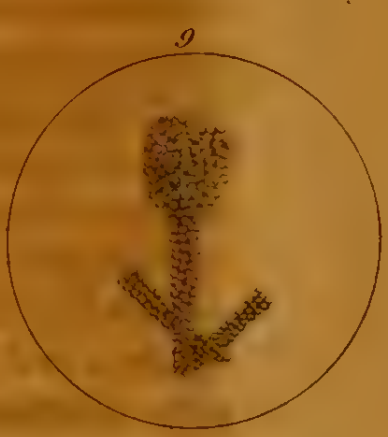
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have reſolved the queſtion concerning the real origin of the moſt common mould. The ſubject has not before been completely diſcuſſed; and it has led ſome perſons into the ancient and dangerous error of ſpontaneous generation.

TRACTS

ON

ANIMAL REPRODUCTION,

BY

THE ABBÉ SPALLANZANI

AND

M. BONNET.

RESULT OF EXPERIMENTS

ON THE

REPRODUCTION OF THE HEAD OF THE GARDEN SNAIL,

BY

THE ABBÉ LAZARO SPALLANZANI.

MEMOIR I.

ARTICLE I.—ANATOMICAL DESCRIPTION OF THE HEAD OF THE SNAIL.

That the learned and curious reader may the more readily be convinced of this admirable reproduction, it will be moſt uſeful to demonſtrate, that the parts compoſing the new regenerated head are not in the leaſt reſpect different from thoſe of the old one cut off. But we are unable to accompliſh this, until with the aid of anatomy, we become acquainted with the parts conſtituting the head

head of the ſnail, which are much more numerous and complicated than at firſt ſight could be credited. (1).

When one of theſe reptiles (2) extends as far as poſſible from the ſhell, it exhibits the whole neck and head. From the anterior part of the latter proceed the four horns, that is, the two larger above and the two ſmaller below. When completely elongated, each terminates in a globule; but in the larger is a black point, generally thought to be the eye. Immediately below the ſmaller horns, the lips appear; and when they open, while the animal feeds, the teeth are ſeen. All theſe parts, as well as the neck, are covered with minute glandular granuli, ſomewhat ſimilar to thoſe of a ſtrawberry, or like *ſhagreen*. But this ſhagreen on the horns and lips is finer than

(1) This Memoir is extracted from my work on *Animal Reproductions*, which would have been publiſhed before now, if ſome engravings eſſentially neceſſary for underſtanding it had been completed.

(2) The Linnean gentlemen will pardon me for not being diſpoſed to claſs ſnails with worms, as their reſpectable maſter does. This is not the place to ſhew how little that opinion correſponds with nature. It will more properly fall within the limits of my work, when complete, where, among other things, will be a diſcuſſion whether the modern theoriſts and nomenclators have been the moſt uſeful to natural hiſtory.

than on the head and neck. The under part of the ſnail is not ſhagreened; on the contrary, it is very ſmooth and ſlippery. Some naturaliſts call it the foot, and not improperly, from ſupporting the animal in its progreſſion. This is all that the eye can perceive: we are obliged to uſe the ſcalpel for penetrating the interior of the head, which is the object of our reſearch. A ſnail cannot be diſſected alive: however much it is extended, it contracts entirely at the ſlighteſt touch, and, retiring precipitately into its dwelling, lies in concealment there. On breaking the ſhell for examination, the head and horns are found retracted in ſuch a manner within the body, as renders it extremely difficult to make obſervations on them conveniently. The eaſieſt method to follow is that propoſed by the great Swammerdam, in his excellent treatiſe on ſnails, which is killing them ſlowly in water: then they almoſt always remain with the head and neck extended from the ſhell, and the horns protruded. In ſuch an advantageous ſituation, when the ſkin of the head is longitudinally divided with fine ſharp ſciſſars, there immediately appears the brain divided into two lobes; from the under part of which originates the medulla oblongata, and from above, the nerves: four are inſerted in the four horns, and extend to the extremities; the other ſix divaricate to different parts of the body, as

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the muſcles of the ſkin, the mouth, throat, and palate.

As far as I know, Swammerdam was the firſt who obſerved that the brain of the ſnail is moveable, and that its mobility ariſes from ſome muſcles to which it is attached. By means of theſe it is drawn towards the fore part of the head, or is extended, according to the different motions of the body. When the animal ſtretches conſiderably from its ſhell, the brain is commonly ſituated above the œſophagus.

It has already been obſerved, that the black point at the extremity of the horns is commonly thought the eye; and ſuch an opinion ſeems very reaſonable. Indeed the ſingular ability of the often named Dutch naturaliſt, and the aſſiſtance of powerful magnifiers, was able to diſcover the principal parts that characterize the organ of viſion; the uvea, the three humours, and the arachnoid, inveſting the chryſtalline lens. I have ſucceeded in ſufficiently diſtinguiſhing all theſe parts, except the aqueous and vitreous humours, which it has never been in my power to diſcern clearly; but this I rather aſcribe to my inability in examinations ſo minute, than to the non-exiſtence of the parts. Of the four nerves, proceeding from the brain to the horns, two, which we ſhall denominate the *optic*, are attached to the eyes, and enlarge into a kind of gourd or rather

rather pear-ſhaped figure below. Theſe nerves have muſcles, by whoſe action, at the animal's pleaſure, both large and ſmall horns are retracted and concealed in the body; and the eyes, by this means, are alſo concealed, to be ſecure from external injury.

On removing the brain, the œſophagus is diſcovered, which is membranaceous, and furrowed by the fineſt longitudinal ridges of livid aſh colour. The ſides are extremely ſmooth; and it contracts as it gradually approaches the mouth. This opening, by which the ſnail feeds, is provided above with a palate and a callous jaw, to which is firmly fixed a tooth of a corneous ſubſtance, cheſnut coloured and ſhaped like a creſcent, terminated by ſome ſharp prominent points, which form, in a certain manner, ſo many moſt minute teeth, though, properly ſpeaking, the animal has only one; and this is therefore the ſnail's only tooth. In the lower part of the mouth is the tongue, provided with a ſmall kind of corneous ſubſtance at the extremity; and the root is fixed in a hollow ſemicircular cartilage. Theſe are the chief parts of the head, omitting a number of muſcles moving them: and as the deſcription of theſe does not ſeem important, I ſhall neglect it altogether, without committing any fault by the omiſſion.

ARTICLE

ARTICLE II.—REPRODUCTION OF THE HORNS, OR ANTENNÆ.

When I firſt diſcovered that ſnails regenerated the horns and ſevered head, ſeveral philoſophers enquired what motives or reaſons induced me to ſuppoſe theſe parts could be reproduced: and it is not improbable that the ſame queſtion may occur to ſome of my learned readers, to whom I ſhall briefly anſwer, as I have hitherto done. Previous to engaging with the reproductions of ſnails, I was employed with thoſe of earth worms, concerning which there is a chapter in my *Prodromo ſopra le Reproduzioni Animali*, and often had occaſion to obſerve how much the reproduction was promoted, when protected from the injuries of the open air, by remaining covered in moiſt earth or dung. This very ſimple remark recalled to my memory the ſtate of ſnails mutilated in certain parts of the body. I had ſeen them ſhut themſelves up within their portable dwellings, cloſing the mouth with that viſcous ſubſtance which exudes from the body, ſo that the air could with the greateſt difficulty get acceſs. I then reflected, that a mutilated ſnail, retired within its ſhell, and ſhut up with the covering, was in a ſimilar ſituation to decapitated earth worms depoſited in moiſtened earth. This rendered

rendered me desirous to examine whether the same would happen to snails as to worms; and my experiments began with cutting off the horns, which, every one knows, are an appurtenance of the head.

That the section of the horns may be complete, it is material they should be fully protruded from the head, which ensues when the snail stretches considerably from its shell; the whole four then being extended, they may be cut out by the root. If two are divided,—the larger for instance, the smaller are instantly retracted within the head, and the snail partly withdraws into its shell; but, in general, it soon appears, displaying the smaller horns. Hence, the experimentalist may cut off all four one by one, if he chuses. Though the animal is so mutilated, the head and neck are frequently protruded from the shell the same as when untouched.

A drop, and sometimes a little stream of transparent fluid, tending on cerulean colour, proceeds from the part where the scissars divide the horns: this arises from the rupture of the glandular substances. In examining the head, now deprived of horns, we discover four pointed trunks; the points being occasioned by corrugation and contraction of the root of the horn, where it has been cut. If we turn to the separated horns, which adhere to the scissars, we observe

that, immediately after diviſion, they ſwell, becauſe they become conſiderably ſhorter. The ſkin, where divided, either corrugates ſo much that the plane of ſection is imperceptible, or it enlarges to cover the optic nerve and muſcles moving the horns. The black eye continues viſible at the extremity of the larger horns, after diviſion; ſometimes, however, it diſappears not, but it remains in the ſeparated horn: it is retracted and buried within, as diſſection proves.

The parts of ſeveral animals, we know, continue to move and live after ſeparation, for a given time. Such is the caſe with ſcolopendræ, earth and water worms, cut in pieces; but particularly, the tails of lizards and water newts, which, for ſome time, will move, bend, and leap about, though cut into ſeveral parts. But quite the reverſe enſues with an immenſe number of animals, ſo that the members, whenever ſeparated from the body, loſe all ſemblance of life and motion. Snails are akin to theſe. Scarcely are the horns cut off when they become motionleſs, or are only ſlightly convulſed for a few ſeconds. No ſymptoms of life are afterwards evident, when ſtimulated with a point.

If the mutilated ſnails are examined in twenty or twenty-five days, it is not uncommon to find the rudiments of a reproducing horn. But this reproduction is very different from that obſerved

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in other animals: and here is one of thoſe uſeful illuſtrations which teach us to diſtruſt analogical reaſoning. The famous Reaumur firſt ſhewed, that the principle of reproduction, in the limbs of the freſh water cray fiſh, began with a little cone in the centre of the trunk, whoſe baſe was infinitely ſmaller than that of the trunk, and, by the proceſs of time alone, became equal to it. A ſimilar phenomenon has been obſerved by the celebrated Bonnet, in his earth and freſh water worms. The ſame appearances have been exhibited to me by the tadpoles of frogs, and by water newts, in reproducing the tail and limbs (1). The rays of ſea ſtars, whether caſually deſtroyed by the bite of an animal, or cut off by men, protrude a little cone or tongue from the middle of the trunk, which is the expanding germ of the defective portion. And, in my voyage in the Mediterranean, during ſummer 1781, I ſaw ſeveral ſtars, that had loſt the rays, budding theſe cones of different ſizes; particularly, the *aſterias rubens* of Linnæus; ſeveral of which I preſerve in the great Muſeum of Natural Hiſtory, in the Univerſity of Pavia. But the truncated horn of a ſnail does not advance in this manner. The trunk itſelf rounds into a little button of a bluiſh colour, which becomes larger and the colour darker; and at the ſummit, if we ſpeak of the

(1) Prodromo. Citat.

larger horns, is a prominent black point, which is the eye. The reproduced part continues extending, and, in a ſhort time, the new horn equals in ſize its unmutilated fellow. In the ſame manner are the ſmall horns reproduced.

If a half, third, or fourth part is cut off, inſtead of the whole horn, as has hitherto been ſuppoſed, reproduction equally takes place, in the like circumſtances as we have mentioned. This is the more common courſe of nature in reproducing horns: ſometimes, however, the trunk becomes long and pointed, inſtead of round. The point in time enlarges, and forms into a globule; the reſt proceeds on as already related.

The ſnail makes the ſame uſe of the new horns as it did of the old, whether by protruding them from the head, extending, contracting, or concealing them, or by diſplaying their acute and lively ſenſibility; ſo that, on the moſt gentle touch, they are ſuddenly withdrawn and put in ſafety.

All theſe facts ſeemed to aſſure me, that the number of parts, conſtituting the divided part, would be exactly the ſame in the portion regenerated. But, wiſhing to aſcertain the truth by the moſt minute anatomical examination, with a very fine iron inſtrument, I laid open ſeveral reproduced horns; however, I could not diſcover the ſmalleſt difference between the new and the old. The ſame ſkin, externally ſhagreened and within

full

full of glandules, appeared; the ſame muſcles moving the horns; the ſame nerves proceeding to their extremities, and there enlarging into an oval bulb; in ſhort, the ſame parts compoſing the eye: therefore it would have been impoſſible to diſtinguiſh the new horns from the old, if I had not witneſſed their origin and increaſe, and if a ſlight contraction of the ſkin had not ſometimes remained at the place from whence they began to ſhoot, or a little projecting eminence which marked the preciſe ſpot where the new horn had originated.

A ſufficient degree of heat is eſſential to the ſucceſs of this reproduction. Temperate is not enough; and the heat muſt be at leaſt 61°. Therefore, in Lombardy, and different parts of Italy, the experiment ſhould be commenced in the beginning of ſpring. What I ſay of heat, reſpecting reparation of the horns, alſo applies to the head, of which I am about to treat. As the ſummer then approaches, the horns are quickly repaired. With regard to the time requiſite for complete reproduction, two months in general ſuffice for whatever part of the horns is to be regenerated. It may alſo be remarked, that although the reproduction ſeldom fails, I have ſometimes been unable to obtain it, notwithſtanding the mutilated ſnails were kept whole years.

ARTICLE III.—HALF THE HEAD REPRODUCED.

We have seen that the conſtituent parts of the ſnail's head are a ſhagreen ſkin, two lips and two mandibles, with a lunar tooth fixed in the upper one: the tongue inſerted into a ſemilunar cartilage, part of an œſophagus; the brain divided into two lobes, and ſending forth ten nerves, beſides four horns of different ſizes. Theſe practical remarks, however, are ſufficient to aſcertain that our experiments, on the decapitation of theſe reptiles, have been correctly executed. If all the various parts formed a head ſimilar to that of moſt inſects, I mean of a globular figure, or were comprehended in a part eaſily diſtinguiſhed from the reſt of the animal, one could at once ſee where it began and ended; conſequently the exact place where the ſciſſars or knife ought to be applied would be known without any hazard of error. But the head of ſnails is otherwiſe conſtructed: when out of the ſhell, the body, if we omit the horns, is a rude figure of a cone, of leſs diameter before, and of greater behind, where confined by the opening of the ſhell. It is a certain fact the head exiſts in the anterior part of the cone, but the difficulty conſiſts in aſcertaining the preciſe portion which it occupies, that we may be ſure how much it is ſafe to cut off. To acknowledge the truth, no abſolute rule can be given, on account

count of the continual extenſion and contraction, the ſwelling and diminiſhing of the cone when the ſnail is in motion. I have commonly found, that the head extends from the obtuſe extremity of the cone to about a line beyond the larger horns, when the ſnail ſtretches to the utmoſt from the ſhell, and there the ſection may be made ſo as ſecurely to take away no more than the head. But if theſe limits be paſſed, ſo as to cut off ſome part of the body along with the head, then we are almoſt certain of the animal's death.

The ſnails on which my experiments have been made with the greateſt ſucceſs are of three ſpecies; the *Helix pomatia*, *nemoralis*, and *lucorum*, to avail myſelf of the appellations of the nomenclator, Linnæus. Several of one ſpecies are deſigned, Plate 6. Fig. 1. The ſnail in its ſhell, but preparing to come out. Fig. 2. It begins to diſplay the head and horns. Fig. 3. The horns fully extended; the ſnail ſeen from below. Fig. 4. The ſnail as far out of the ſhell as poſſible. Fig. 5. A portion of the cone comprehending the head only, which, for better illuſtration, is repreſented ſevered from the neck. Fig. 6. The decapitated ſnail: four points appear on the trunk denoting the ſite of the four horns.

Having obtained reproduction of the horns, it occurred to me to examine what would enſue on ſevering the head; but apprehenſive that the ſnails would die before repairing the whole, my experiments were begun with truncating a part only, which was that free of the larger horns, and comprehending the lips, the mandible, and tooth, the tongue and the two leſſer horns, beſides the muſcular involucra or integuments. This portion I ſhall hereafter call the *half cut head*, for the ſake of brevity, fig. 4. a. c. That the diviſion may be properly completed, it is requiſite for the ſnail to be fully extended, and the part then cut clean off with ſciſſars, making the ſection perpendicular to the axis of the cone. But the operation has not uniformly ſucceeded according to my deſire. The head of the ſnail, as already obſerved, is extremely ſenſible, and ſcarcely is the touch felt, when it ſuddenly contracts and retires: or, if it is extended, it turns about in various directions. Thence the ſection often fails of ſucceſs, if it is made obliquely, and the half head is not exactly ſevered. But as I judged it of the utmoſt importance, to the accuracy of the experiment, to learn what parts only were cut off, that it might be known whether the whole would afterwards be reproduced, ſhould ſuch a reproduction actually exiſt in nature, I impoſed a labour on myſelf, which I had not done

done in my experiments on the reproductions of other animals; and this was an anatomical examination of every half head, immediately on separation. I put the decapitated snails into so many small vessels, each numbered the same as in my journal, along with a brief anatomical description of the parts of each divided head. Thus there was no danger of mistake in my experiments; and I could also learn whether the parts repaired corresponded perfectly in figure and size to the parts cut off. After these remarks, which it is proper to make, we may pass to the narrative of the results.

Immediately after amputation, the snail retires with the greatest precipitation, to conceal itself in the shell; and frequently, in the act of retiring, emits a gentle hissing, which arises from the difficulty which the air finds in escaping from the respiratory canal, that being in some measure contracted by the sudden contraction of the body. Notwithstanding this enormous wound, the snail sometimes soon afterwards comes out, and begins to crawl about in the same manner as when untouched. A snail having lost half the head, and then proceeding from the shell, is designed, Fig. 7. The two points r s mark the situation of the small horns. The large are less than usual, because not fully extended. But the reverse generally happens. However, there is a very easy method

method of forcing the ſnail out for the purpoſe of viewing and examining the wounds, which is, breaking a little of the ſhell behind with repeated gentle ſtrokes of a key or the handle of a knife: the ſnail, irritated by the blows, appears in whatever degree of reproduction the head ſhould be. To avoid uſeleſs repetition, I ſhall here obſerve, that ſuch expedients have prevailed whenever the ſnail refuſed to appear, and gave me an opportunity of ſeeing the cut if lately made, or, indeed, the rudiments and progreſs of the reproducing heads. When the portion is cut off, ſome of that liquid, which in ſnails ſupplies the place of blood, eſcapes: it ſoon ſtems, becauſe the trunk immediately begins to corrugate and diminiſh, ſo as almoſt to diſappear; and in its place is obſerved a ſlight incavation, where no marks of the cut are viſible. The ſnails thus decapitated for the moſt part fix to the veſſels containing them: they conceal themſelves in their habitations, covering the mouth with their whitiſh operculum, produced by the tenacious matter which exudes from the body, and there they remain motionleſs for many weeks, or even for complete months.

When forced to come out, in thirty or forty days, the naked trunks of ſome appear without any marks of reproduction, but others, if the weather has been warm, exhibit a fleſhy globule towards

towards the middle of the trunk, very ſoft, and of a whitiſh aſh colour, in which there is no organization, either without or within. However, in eight or ten days more, organization is ſenſible in the globe, then become much larger. The rudiments of the lips are evident, as alſo thoſe of the ſmall horns, the mouth, and tongue, and a membranaceous dark coloured ſubſtance, which, from being fixed in the upper jaw, and cutting through, ſhews it to be the regenerating tooth of the ſnail. Theſe parts develope further, and grow more conſpicuous; they ſucceſſively occupy greater ſpace in the trunk; and, in two or three months at moſt, the divided head is repaired in ſuch a manner, that, unleſs from the lighter colour, it is not diſtinguiſhable from the old one. This, beſides external inſpection, is demonſtrated by anatomy. When the new head is laid open, the ſame parts are ſeen, correſponding in number, figure, and ſize, to thoſe pre-exiſting in the old, which were ſcrupulouſly enumerated in my journal of each decollation. I cannot convey a more ſenſible idea of this reproduction to my readers, than by comparing it to an unexpanded flower. Conſidering the rudiments, they are a bud or little globe, conſiſting of membranes, ſo involved and aggregated among themſelves, that we cannot diſcover the figure of the leaves or petals, as they may be called. Theſe petals gradually

dually come into view, at firſt obſcurely and confuſedly, then ſo diſtinctly and evident, that every one may recogniſe them as the bud of a flower. In this manner does the involution of the expanding parts become viſible in a real regenerated head.

This complete reproduction is very far from ſucceeding in all ſnails. Two moſt minute globules often proceed from the trunk, in one of which are the rudiments of the ſmaller horns: the other comprehends the rudiments of the lips, the mouth, tooth, and tongue. In proceſs of time, theſe globules are united together, forming one only, and, by further unfolding, conſtitute the half head. It is not unuſual, that one of the two reproduced horns never attains the natural length, or is diſtorted, or that one lip is ſmaller than the other, or even that the new head is quite inclined to one ſide, or a hollow or contraction between the new and the old, or, in ſhort, that the head is not repaired at all; and after ſix months, nay, after a whole year, the naked trunk alone appears, when the ſnail comes forth. When the cut is perpendicular to the axis of the cone, I have almoſt uniformly obſerved, that reproduction has perfect ſucceſs, and that monſtroſity and ſuch anomalous productions frequently happen when the cut is oblique, and

and the bluntneſs of the ſciſſars prevents the head from being at once divided.

I half decapitated three hundred and twenty two ſnails at different times: the heads of one hundred and twenty ſix were completely repaired. Thirty one had different degrees of monſtroſity or deformity. Fourteen had no reproduction, and the remainder periſhed (1). Thus I have learned, that cutting off only half of the head was fatal to moſt ſnails. All thoſe that repaired the wanting portion, made the ſame uſe of it as of the old, as well in the numerous and ſingular motions peculiar to this part, as in taking the ſuſtaining aliment, ſuch as bread, lettuce, and the like; by which means, from being very much emaciated before reproducing the head, they acquired their original fleſhy fullneſs.

ARTICLE IV.—REPRODUCTION OF THE HEAD.

In the former ſection, we have ſeen that the complete head of the ſnail extended about a line beyond the large horns. In this new courſe of experiments, I determined to ſeparate the head entire, and endeavoured to make the cut exactly in

(1) I diſcovered that half, as well as the whole head, would be reproduced during ſpring and ſummer 1766.

in that place, and perpendicular to the fleſhy cone formed by the head and neck. But the ſame difficulties are found here, as in amputating half the head. Thus the motion of the animals prevented the cut from always being made in the place intended. It has often failed, either from defect by ſeparating leſs than the whole head, or from exceſs, by ſeparating more. Therefore, that proper accuracy might be obſerved at every decapitation, I undertook the ſame anatomical examinations as before: the ſnails were preſerved in veſſels appropriated for the purpoſe, as alſo an exact account of the parts they had loſt by amputation. Four hundred and twenty three were decapitated.

All thoſe that, along with the head, had loſt part of the neck, periſhed; nor is this ſurpriſing, conſidering that a portion of the organs of generation was likewiſe taken away. Theſe organs originate within, in one ſide of the neck, and protrude by an opening when the animal copulates. A number mutilated of the whole head periſhed; but moſt of them ſurvived this immenſe wound, and many completely reproduced the head. But as reproduction was attended by different circumſtances, in various individuals, and all worthy of being known, it ſhould be deſcribed in a more particular manner.

If a limb is cut off a water newt, the head or tail from an earth worm, the reproduction that enſues

enſues is an organized whole, that is, a limb, a head, or a tail, in miniature, perfectly ſimilar to the ſevered one, and only requiring to be farther unfolded. On the contrary, no organized whole, comprehending all the parts of the ſevered head, appears on the trunk of a decapitated ſnail, but theſe parts are frequently ſeparate from the beginning. Thus, ſome frequently expand after others; and only in a certain ſpace are they all connected together, conſolidated, and forming an organic whole, different in little or nothing from the old head. The ſubject will be more eaſily underſtood by examples.

Sometimes the incipient reproduction is a fleſhy protuberance, adhering to the middle of the trunk by ſeveral points, and in a manner detached from it, which contains the rudiments of the two lips, the ſmaller horns, the mouth, tongue, and tooth, already repaired. The other parts, ſuch as the larger horns and the reſt of the head, are wanting altogether. The trunk of another ſnail will exhibit a large horn, already provided with its eye, and below, in a diſtant iſolated part, are obſerved the firſt lineaments of the lips. In others, the reproduction is a groupe of three horns, two already of their natural ſize, and the third only a bit of ſkin. At firſt, ſome produce only a protuberance, which, by attentive

tive examination, is difcovered to be the lips involved and confined together. Some are already provided with a head complete, all except one or two horns. Laftly, the trunk will exhibit only the two large horns, or the fmall; or one large and one fmall.

But all thefe partial reproductions, and others that afterwards appear, join together in procefs of time, and by their union form a fingle reproduction, which is the head, and this, in many fnails, is not in the fmalleft degree different from the old one, except in the lighter colour; by means of which the leaft obfervant perfon can recognife the portion reproduced. One that has completely repaired the head is repreferted, fig. 8. It has not yet acquired the dark natural colour. Fig. 9. repreferts the fame, only this fnail has not yet repaired the two large horns, as fometimes happens. In a little more time, the new head acquires the fame hue as the old, and the one can be diftinguifhed from the other, only by an afh coloured line, perpendicular to the axis of the neck, which faithfully indicates the place where the blade has paffed in mutilating the fnail. This is not conftantly a fimple line, fometimes it is a deep hollow, almoft always of a whitifh colour, perpendicular to the neck, if the cut has been perpendicular and oblique, if the cut has been fo. In the latter cafe, the incavation is frequently

ly greater where moſt part of the head has been cut off: and, in ſome ſnails, an enormous wound appears on one ſide, though nothing is viſible on the other, or only the aſh-coloured line. And although length of time effaces the incavation, ſtill the indication of the cut, that is, the line, will ſometimes remain two years: nay, even after ſo long an interval, the head is not always complete, for it may want one or more horns; or theſe, at leaſt the whole, have not attained the proper ſize, or are gibbous and monſtrous. Such monſtroſity having frequently occurred, I am inclined to ſuſpect that it originates from the obliquity of the cut, or from being more or leſs advanced on the neck.

The moſt indubitable proof of regeneration ſeemed to be when the heads began to feed. However, I was deſirous of convincing myſelf by the infallible aſſiſtance of anatomy, which has always demonſtrated that the new heads, which externally ſeemed to be completely reproduced, were provided with all the conſtituent parts that I had found in the old heads, which, in each decapitation, had been enumerated to avoid doubts and errors. I may add further, that each new part united, and ſo exactly applied its moſt ſubtile fibres to the old, that we ſhould never have known the ſnails were mutilated, had it not been

indicated by the aſh-coloured line ſurrounding the neck.

Here I ſhould not neglect to obſerve, that, in the ſame manner as ſome ſnails, deprived of half the head, never reproduced even in the longeſt time, the like has equally happened to ſeveral of thoſe totally decapitated. Indeed, of four hundred and twenty-three mutilated in this way, thirty-two did not evince the ſmalleſt rudiments of reproduction in a year; ninety-three could not reproduce better: the heads of one hundred and forty-five were regenerated with monſtroſities; and the remainder died. Reproduction of the whole head requires nearly the ſame time as that of the half.

Should I be aſked, why there is no reproduction, either of the head or horns, in different ſnails? I ſhall ingenuouſly confeſs myſelf unable to advance any thing concerning it but ſimple conjectures. As the reproducing and unreproducing ſnails are both of the ſame ſpecies, we cannot ſay that ſome of them have the property and others have it not. I ſhould rather ſuppoſe, that the reproductive virtue cannot take effect from the diſeaſed ſtate of the ſnails; having uniformly obſerved, that, beſides their moſt remarkable emaciation, the exterior aſſumes a yellowiſh hue, which ſeems the inſeparable concomitant of ſnails affected by diſeaſe, and liable to periſh.

After

After obtaining reproduction of the head, it was very natural to think that ſnails would regenerate other parts leſs eſſential; ſuch as, the eminent collar which ſurrounds and ornaments the back of the animal, when out of the ſhell, and the flat broad foot by which it is ſupported during its motion. Theſe two parts, when cut off, were repaired in the beſt poſſible manner; nature reproducing much or little, according as it had been cut away.

Uniting, in one point of view, what we have hitherto ſaid of the reproductions of ſnails, it clearly appears, 1. That they can repair the horns, whether partially or wholly amputated; 2. The head, when half is cut off; 3. That it is as completely reproduced, though the whole has been ſevered; 4. Whatever part of the collar or foot is cut off, it is regenerated. From which it is evident, that theſe reptiles recover preciſely the parts that they have loſt. This is not peculiar to them, for it takes place in other reproducing animals. If a newt wants a third or fourth of the tail, only a third or a fourth is reproduced; and the ſame may be ſaid of one half, or the whole tail. A ſimilar phenomenon ſucceeds in the fore and hind legs of this amphibious animal (1). Reproduction of the parts wanting only, extends invariably to worms, as alſo to frogs, while yet

(1) Prodromo citat.

tadpoles. Beſides, on conſulting the authors that have treated of animal reproductions, I find that the ſame unalterable laws have been obſerved; whence we may eſtabliſh this general rule, if ſome anomalies are excepted, *In animals, endowed with a reproductive property, nature reſtores no more than the parts or organs of which they have been deprived.*

ARTICLE V.—FACTS RELATIVE TO THE REPRODUCTION OF THE HEAD OF SNAILS.

This Memoir ſhall be terminated with ſome new experiments, which I hope will not be diſagreeable to the reader, as they will elucidate this intereſting ſubject. Hitherto three ſpecies of ſnails have been mentioned, the *pomatia*, *nemoralis*, and *lucorum*. But are theſe the only ſnails enjoying reproductive powers? The experiments of foreign naturaliſts demonſtrate that other ſpecies participate the ſame privilege. Yet I muſt acknowledge, that my experiments on others have almoſt always been void of ſucceſs, for they have either not repaired the head or horns, or only exhibited the firſt rudiments of reproduction, which terminated with the death of the animal.

But whence does it ariſe that ſome ſpecies of ſnails have the faculty of reproduction, and others have not? Should ſuch a queſtion occur, I muſt candidly confeſs, that I have had not ſufficient

ſufficient materials to explain it. Was the organic ſtructure of the reproducing ſnails very different from thoſe that do not reproduce, ſome reaſon might be aſſigned for it; or, to ſpeak more philoſophically, was there any diſparity between them. However, no ſuch thing was found, at leaſt by me, with any certainty, among the various ſpecies of ſnails that either ſucceeded or did not ſucceed in reproducing.

This uniformity of organization in ſnails ſpecifically different, which at the ſame produces oppoſite reſults, as reproducing and not reproducing, is a uſeful leſſon that we cannot avail ourſelves of analogy in reaſoning from one ſpecies to another; but that truth may be attained, we are under the neceſſity of undertaking as many experiments as there are animals ſpecifically different; and the force of this concluſion, ſo humiliating to us, is more plain and evident, by throwing a tranſient glance on the various animals which naturaliſts have at different times diſcovered to be endowed with a reproductive virtue.

When the immortal Trembley firſt diſplayed the prodigies of the polypus to the philoſophical world, it was thought that the very ſimple ſtructure of the animal chiefly contributed to effect them. Indeed, the polypus being deſtitute of a heart, veins, and arteries, and, conſequently, of the real circulation of fluids; neither brain, ſpi-

nal marrow, or nerves, nor any other concomitant of these parts, which are met with in an infinity of animals, being found in it; but the whole appearing of a gelatinous and homogeneous substance, universally covered with a number of granules; all this, I say, excited belief, that the simplicity of structure concurred in marking the portion cut from the polypus become an entire polypus. Thus it was thought, before the Genevese philosopher's discovery, that the wonderful phenomena, afterwards seen in polypi, succeeded in plants from their very simple structure. And the reproduction of parts lost by other animated beings of simple structure, as sea nettles, sea stars, and I may also add, crayfish, favoured the opinion. But it having afterwards been found, that certain fresh water worms, though much more compound than polypi, when cut in pieces, would become so many complete worms, it was demonstrated, that simplicity of organization was not a condition requisite for the reproduction of wanting parts (1). Besides, this is verified in the clearest manner by earth worms, since Reaumur has found that, cut in pieces, they multiply like plants, which is a physiological fact that some naturalists have denied, but it will be put beyond all question in my *Riproduzioni Animali*. When

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(1) Bonnet Traité d'Insectologie.

I name the earth worm, I ſpeak of an animated being whoſe organization is a thouſand times more complicated than that of the polypus, from finding the circulation of blood, and, conſequently, arterial and venous veſſels in it: an alimentary canal, ſpinal marrow and nerves, and the union of two ſexes, as it is an hermaphrodite. Of the ſame complicated ſtructure is my freſh water boat worm; nevertheleſs its reproductive faculty is not inferior to that of the earth or freſh water worm (2).

Yet how much higher does the organic ſtructure of ſnails and newts place them in the animal ſcale? Let us omit the former, as enough has already been ſaid of them, and ſtop a moment to conſider the latter. Though naturaliſts have properly claſſed the water newt among amphibia, it is a real quadruped, as it poſſeſſes in miniature moſt of the parts which quadrupeds have large. A great number of theſe have the tail provided with oſſeous vertebræ inſinuated into each other, and ſucceſſively ſmaller as the tail diminiſhes. The tail of water newts is of the ſame configuration, and conſiſts of the ſame oſſeous vertebræ: it alſo has the ſoft ſolid parts, ſuch as the *medulla longa*, which traverſes each vertebra, and perforates the ſmalleſt: it has nerves, muſcles, veins, ar-

 teries,

(2) Prodromo.

teries, a heart, a numerous aſſemblage of glands containing that acrid milky fluid which exudes through the ſkin when the newt is irritated. Both the fore and hind legs have nearly as many ſoft ſolid parts, and hard ſolid parts or bones, as thoſe of quadrupeds, and almoſt as we have ourſelves. Finally, as a rudely circular bone ſurrounds and terminates the mandibles of our amphibia, from which a little regular foreſt of the ſharpeſt teeth projects, Who could have ſuppoſed that this quadruped had the property of reproducing ſuch an aſſemblage of parts ſo different among themſelves? But it is moſt undoubted: for, beſides being the firſt diſcoverer myſelf, I have witneſſed it repeatedly. Loſing the whole four limbs at a blow is nothing to a newt, becauſe it can reproduce them all, and reproduce them perfectly. I have taken the trouble of numbering the bones in theſe limbs, and found them to be ninety-nine: and ninety-nine have exiſted in the four reproduced limbs when all were amputated from the trunk. Nay, let the four legs be completely cut off, and the whole tail, as alſo the two mandibles; the newt, in addition to reproducing the limbs, will at the ſame time repair the jaws and the tail. This fact, which has ſo much the appearance of a paradox, and at firſt ſight ſeems more fabulous than the famous Lernean Hydra, I have repeatedly ſeen

and

and ſhown to many friends, to the great amazement of them all. The water newt is ſo much more the object of admiration, ſince it never defrauds the eager experimentaliſt of its multiplied reproductions, which is otherwiſe with ſnails, as ſome of them will not reproduce.

Behold the evident exiſtence of reproduction, beginning with the polypus, proceeding to various worms, then to ſnails, and, laſtly, to water newts, that is, advancing from the moſt ſimple animals to others leſs ſimple, and from theſe to ſome whoſe organic ſtructure is more complicated; and there is no eſſential difference produced by the more ſimple or more complex organization.

Theſe facts alſo prove, that the tenderneſs or delicacy of fibre is by no means a condition neceſſary for animal reproduction. How great is the difference between the body of a polypus and the tail or limbs of a water newt? But do not both reproduce in the ſame manner? How many ſmall animals are there, as delicate as the polypus, even much more ſo, and as completely aquatic, which, inſtead of reproducing when cut aſunder, inevitably periſh? as I have aſcertained by numerous experiments.

However it is proper to remark, that of the animals adapted by nature for reproduction, thoſe provided with the more tender fibre have a ſingular

gular prerogative over the reſt. In the firſt place, the defective limbs begin ſooner to be repaired. A polypus, divided into many pieces, in a few hours begins to multiply into ſo many other polypi; an earth or water worm requires a few days; a ſnail or newt, on the contrary, requires ſeveral weeks, before beginning to reproduce. Secondly, reproduction is much ſooner complete in the former. Only a few days are neceſſary for a polypus; worms require whole weeks; the ſnail muſt have ſeveral months for repairing its head; and a year is not ſufficient for the new limbs of a newt to grow as large as the old. Thirdly, the ſame animal, ſo long as young, and the fibre conſequently more tender and pliant, will reproduce the loſt parts quicker. This I have ſeen in newts, and alſo in ſnails, which will repair the ſevered head in ſix weeks, and much ſooner, if young. Finally, reproduction is more tardy, as the natural ſoftneſs of the animal decreaſes. We have a ſtriking inſtance of this in frogs. If, while ſtill tadpoles, but the limbs beginning to appear, their limbs ſhould be amputated, by my own obſervations it is certain that they will be moſt completely repaired. But the ſame will not ſucceed when the tadpole has aſſumed the figure of a frog: then it is never, or almoſt never, that the trunk puts forth a new limb. Whence ariſes ſo great a difference in the

Fig. 1.

the same limbs of this amphibious animal? Shall we say, that this virtue, this reproductive power, which the animal enjoyed while a tadpole, has been lost by it becoming a frog? as if, by the metamorphosis, it ceased to be the same animal, which is but an unphilosophical sentiment (1). I find it more consistent with truth, to suppose the reproductive power continues in the frog; it is enabled to operate in the tadpole, by means of the great tenderness of fibre, but its action is afterwards prevented from the succeeding induration. Let us endeavour to elucidate this a little. The frog, while a tadpole, never leaves the water; and it would perish on attempting to do so. Only from time to time does it dart from the bottom to the surface, and, for a moment, puts up its mouth, to expel the air from its lungs, and inspire what is fresh. The trunks of the amputated limbs then remain in a state of the greatest softness, being always immersed in water, and bathed by it in every point. The minute limb, yet a germ, will be able to penetrate the trunk, if this expression may be used, to come out and freely expand. But the same thing will not happen, when the frog has attained its full and permanent size. Then, as it generally remains out of the water, or retreats thither only when menaced by danger, the trunk will be subject

(1) Dissertaz. de Fis. Animal. et Vegetab. T. 2.

ject to the influence of the air: thus it will cicatrise; and the contraction, occasioned by the cicatrice, will prevent the reproducing germ from breaking through and expanding. The mutilations of snails, hitherto mentioned, were for the most part performed when spring was somewhat advanced; because I had observed, that no less than 61° of heat was necessary to obtain reproduction, which we generally have not in Lombardy before that time; and if they are then decapitated, it is certain that the snails, at least many of them, will repair the head. But what will ensue, if decollation is towards the middle of September, or when the requisite heat does not continue with us more than a month, on account of the supervening autumnal rains? I have instituted many experiments for elucidating this curious enquiry, and obtained the following results: If the mutilated snails were exposed to the heat of a stove, equal or surpassing the necessary degree, I was sure of reproduction before the end of winter. If kept in a situation where, for some days, they might be exposed to the cold of freezing, the greater part perished. When the cold was less, they retained the power of reproduction, which re-appeared in spring; and the head and horns, having begun to grow at the commencement of winter, attained their full size in the subsequent spring. If decollated in the beginning

ginning of winter, and care taken that they did not periſh with cold, though without being kept in a ſtove, no reproductive principle appeared on the plane of the trunk; but it became evident in May, and advanced to perfection during the ſummer months.

The ſame mode of regeneration practiſed by nature in the reproductions of ſnails ſhe alſo practiſes in thoſe of newts, of earth worms, and water worms; with one variation, however, that theſe animals reproduce, though ſlowly, at the temperate degree, which ariſes either from the great ſoftneſs of their fibre, or from ſomething peculiar in their nature.

When engaged with the reproductions of earth worms, it ſtruck me to try whether the reproductive power was exhauſted by the firſt reproduction, and I found that it was not. Thus to the ſecond reproduction ſucceeded a third; and this being taken away, there came a fourth, then a fifth, and ſo on. If a portion of ſuch ſucceſſive reproductions were cut off, the ſecond reproduction entered the firſt, and the third the ſecond, &c. Thus I came to have a ſcale of reproductions united to the old trunk, always younger, ſmaller, and the colour gradually lighter.

Theſe regenerations of reproductions equally ſucceeded in the tail of tadpoles, and, what is more ſurpriſing, in that of newts, and likewiſe in their

their limbs, though the parts of these two members are so different from each other. Therefore, if the four limbs and tail of a newt, already reproduced, are amputated, other four limbs and a tail will regenerate a second time; and this experiment may be considerably protracted. Indeed, with young water newts, where the reproductions were prompt, I obtained six successive reproductions of the limbs and six reproductions of the tail during the months June, July, and August. In one of these animals, I counted six hundred and eighty-seven bones reproduced.

In consequence of these reproductions in earth worms, tadpoles, and newts, I thought of trying whether they would succeed in snails; and, for that purpose, mutilated several, some of the horns, and others of different portions of the head, or the whole; and I cut off whatever parts were renewed, exactly where they joined the old trunk. The second reproduction did not fail to take place and succeed in the same manner as the first; and this also happened with a third reproduction; but the death of the snails prevented me from extending these curious experiments further.

Another suggestion occurred, besides this enquiry, which was to investigate whether the reproductive powers could at last be exhausted, or whether they would always succeed, so long as the

the animal lived. And the experiment might have been begun on newts in preference, ſelecting the youngeſt, becauſe this amphibious animal is eaſier mutilated than ſnails, more tenacious of life, and reproduces more readily; but I had not leiſure for ſuch experiments, which, if undertaken by any expert naturaliſt, will undoubtedly tend to utility, and may form a new chapter in phyſiology.

In the whole courſe of this memoir, I have delivered the reſults of my experiments only, and always ſuppreſſed the details. Had I related theſe, it would have been a volume, not a memoir; but I was deſirous of inſtructing the reader, without fatiguing him by long circumſtantial narratives. I always hope to have obtained ſufficient credit with the public to merit belief. It gives me pleaſure to obſerve, that almoſt the whole of theſe reſults are confirmed by diſtinguiſhed naturaliſts, as will appear in a ſecond memoir on the reproductions of ſnails, where an abſtract ſhall be given of the writings in my favour, and, at the ſame time, one of all thoſe that attempt to controvert them. The confirmers, as far as I know, are Meſſrs Turgot, Lavoiſier, Tenon, Heriſſant, Bonnet, Senebier, Schaeffer, Roos, Muller, Scarella, Troilo, beſides three other Italians, celebrated profeſſors of anatomy, who, having recently repeated my experiments, and found

found them true, have been ſo obliging as to communicate their memoirs. The controverters are Meſſrs Murray, Wartel, Cotte, Bomare, Adanſon, Schroeter, Argenville, Preſciani. In my ſecond treatiſe, I ſhall likewiſe examine the value of the impugnations with philoſophic impartiality, and ſhall not fail to give the merit of the impugnators its full weight. I am very far from ſuppoſing that my diſcovery will form an epoch in natural philoſophy; it will rather form an epoch in the hiſtory of the human mind, to ſee how an experiment, ſo certain, ſo eaſy as that is, of obtaining the reproduction of the head of the ſnail, has deceived ſuch a number of philoſophers,—and, what is more aſtoniſhing, in an age which ſeems to be that of obſervation and experiment, if, on the other hand, it was not remarkable that experiments are made by every one. But the proper method of experiment has always been, and will always be, confined to very few.

MEMOIR

MEMOIR II.

INTRODUCTION.

I KNOW not whether in our days there has been any natural phenomenon which, from novelty and ſingularity, has made as great a noiſe in the phyſical world, has given birth to ſo many experiments, and altogether to reſults ſo various and oppoſite as the reproduction of the heads of ſnails. Since the publication of my *Prodrome*, and the tranſlation of it into the French, German, and Engliſh languages, it is incredible how many of theſe reptiles have been decollated; partly by ſome whoſe account of their experiments firſt imparted to the world that they exiſted; partly by ordinary naturaliſts, or thoſe of ſome celebrity, but little leſs than unſkilled in experiment, and partly by illuſtrious naturaliſts who, in the art of experiment, enjoy a diſtinguiſhed reputation. How was it poſſible that ſo great a differ-

ence among the experimentaliſts did not produce conſiderable diſcrepancies in their reſults? Thus it has actually happened, that ſome, very far from obtaining the reproduction of theſe teſtaceous animals, have beheld them die, which has given occaſion to diſpute and reject my diſcovery, from conſidering it more imaginary than true. Others, having obtained only the rudiments of reproduction, have not thought it falſe but exaggerated. However, the reſt, by obtaining complete regeneration of the head, have amply confirmed it.

As the principal reſults of my own experiments were narrated in the firſt memoir, I have now to relate thoſe of others: explaining with amicable impartiality, as well thoſe favourable to my cauſe as thoſe that are adverſe to it. But it ſhould be obſerved, that I ſhall only ſpeak of the experiments both for and againſt, that have appeared in print, or which unpubliſhed have been communicated to me by diſtinguiſhed naturaliſts. Although there are other experiments on the ſubject, and theſe neither few, nor made by men of contemptible fame, I do not conſider them deſerving of repetition, becauſe they have not been acknowledged by their authors, who, ſurpriſed at the ſingularity of the fact, had no object in view, good or bad, but to make them, and were indifferent about committing them to writing, much leſs to render them public.

To

To elucidate the matter, and preserve regularity, it will be proper to divide the treatise into three articles: The first will enumerate those experiments that controvert my discovery; the second will comprehend those confirming it; and the third will embrace a few reflections that I have esteemed applicable for removing doubt, and placing the truth in a clearer point of view.

ARTICLE I.—EXPERIMENTS ADVERSE TO THE REPRODUCTION OF THE HEAD OF THE SNAIL.

We may call the *Avant Coureur*, a weekly publication in Paris, the field of battle, where many authors have contended both for the reproductions of decapitated snails and against them. The first opposition was made in 1768. 'M. Wartel, 'about the end of October 1767, decapitated 'many snails, which suddenly retired within their 'shells. With great surprise he saw them issue 'from their dwellings full of life, but without the 'head, in May 1768. He does not imagine that 'the reproduction of the head is possible: since 'none of his repaired it, and some did not even 'renew the horns.'

Father Cotté repeated these experiments, as appears from the same journal, May 1769. In June, taking advantage of a shower of rain, he decapi-

tated a number of ſnails with a pair of ſharp ſciſſars. Four of twelve died in eight days, and only one of the reſt ſurvived: it lived more than a year, but without the ſmalleſt appearance of reproduction. In the ſame month, he decapitated other twelve. Some, which he thought had undergone the operation, ſtill exhibited the horns complete; whence he concluded, that the ſnail may ſo ſuddenly retract the head, that only part of the ſkin or integument is cut off, and thus deceives the obſerver. Thoſe, however, leſs alert, and which really loſe the head, he is fully convinced never repair it.

He continued decollating a prodigious number of ſnails during 1770, 1771, 1772, 1773, and with nearly the ſame conſequences. All died in time without reproducing the head. If the ſciſſars ſeparated only the ſkin or horns, theſe parts were repaired.

In the ſame year, 1769, M. Valmont de Bomare publiſhed his experiments in the Berne Journal. 'He repeated Sig. Spallanzani's expe-'riments along with M. Borie. They have ob-'ſerved, that decapitated ſnails immediately died, 'exhaling an intolerable fœtor. Of 52, only 9 'were alive in 24 hours; and none except thoſe 'in which the bluntneſs of the knife had made 'the cut between the horns and parts of genera-'tion, and did not go through. The horns were

'ſenſibly

'ſenſibly retracted when the ſkin and upper jaw 'were cut off: and the animal, having come out, 'diſplayed the mutilated horns.'

To theſe three naturaliſts ſhould be joined M. Argenville and Schrœter. With reſpect to the former, I have not been able to procure his work: but Mr Murray quotes it in theſe words: 'Of 'an hundred ſnails, hardly 25 were alive the day 'after decapitation.' The German naturaliſt decapitated ſeveral hundreds: as they all died, he thinks that he is authoriſed to deny that theſe reptiles renew the head: and never having had the leaſt part of the horns or tail repaired, he equally denies the poſſible reproduction of theſe parts (1).

There are ſtill ſome authors who do not entirely controvert my diſcovery; for they admit reproduction of part of the head, but deny that of the whole. Theſe are Meſſrs Murray, Adanſon, and Preſciani. Murray mutilated two of the ſpecies *pomatia*, ſo that the head was ſevered behind the large horns. One died in a week: the other reproduced, though in miniature, for it was only a ſmall horn, and as if in deſpite of nature: it was ſhorter and thicker than uſual, and wanting the black point at the extremity, which is

 commonly

(1) Verſ. e. Syſtem. abhandl. uber. d. Erdconchylien. Berlin, 1771.

commonly reputed the eye. He mutilated other ten, in various ways, of the ſpecies *nemoralis*. In about ſix weeks, one, deprived of the horns only, had two reproduced, but both wanting the eye. Thoſe decollated retired into their ſhells, and could not again be forced to appear. Mr Murray's patience being exhauſted, he broke one of the ſhells, and ſaw two points like the rudiments of horns already protruded. A fortuitous event prevented the continuance of his obſervations. From what he had ſeen, he declared his reſolution to preſerve a middle courſe between thoſe who denied the reproduction of the head, and thoſe who admitted it.

Somewhat analogous are the experiments of Dr Preſciani of Arezzo in Tuſcany, inſerted in the *Giornale di Piſa*, 1778. He ends the detail of his experiments thus: 'In my opinion, I have 'been able to aſcertain, that the ſnails, which had 'loſt part of the brain along with the head, died 'ſooner or later according to the portion taken 'away: that the others, which had loſt the 'horns, lips, teeth, throat, and tongue, without 'the brain being touched, lived as long as they 'could ſurvive without food. Thoſe deprived of 'the horns and integuments of the head have all 'had perfect reproduction.' M. Adanſon is alſo in the liſt of experimentaliſts, not that he has publiſhed any writing ſpecifically on the ſubject,

but

but from a letter to M. Bonnet, we know that he has ſacrificed more ſnails than any other naturaliſt, as more than 1400 were decapitated in one year. He had partial reproductions even immediately, of horns, heads, lips, &c. but theſe were reproductions of parts not entirely cut off: ſince all the heads, horns, jaws, &c. completely ſeparated, never manifeſted the ſmalleſt reproduction.

ARTICLE II.—AN ACCOUNT OF THE EXPERIMENTS WHICH CORROBORATE THE REPRODUCTION OF THE HEAD.

NOTWITHSTANDING the multiplicity of the experiments related, I have never found myſelf in the leaſt inclined to repent having publiſhed, in the Prodromo, my ſingular obſervations on ſnails, and that becauſe I was too well aſſured of having ſeen what is ſtated there, and without any apprehenſion that deceitful appearances had impoſed upon me. In the end of ſpring 1766, I had witneſſed the renewal of the head and horns; but I delayed laying this wonderful phenomenon open to the world until 1768. I was not content with having once beheld it; I wiſhed to ſee it anew, and to return to it often, at the ſame time obſerving the moſt minute anatomical

examination, as well of the ſevered parts of thoſe renewed, to aſcertain whether, in all ſtrictneſs, the part reproduced might truly be called the head of the ſnail. In my literary correſpondence with M. Bonnet, I regularly recounted the whole ſeries of facts, reſpecting the beginning, progreſs, and termination of the reproductions, of which he was completely convinced, as appears by many letters to me on the ſubject, now printed in his works. And of the ſame opinion were all the learned perſons, to whom I had ſhewn the reproductions, both in Modena, while profeſſor in that univerſity, and here in Pavia. Even about the end of June 1782, having ſome ſnails that were mutilated in May, and reproducing favourably, I had the pleaſure of ſhewing them to two reſpectable friends, excellent judges of ſuch matters, M. Tiſſot and Father Barletti, who, after examining them ſtrictly in every part, agreed, without heſitation, that the parts repaired were not the *cap* or *bonnet*, as M. Adanſon humorouſly calls it, but true, moſt certain, moſt undoubted new heads. Therefore M. Tiſſot, about the beginning of July, did me the favour to carry ſome of the reproducing ſnails to Geneva, to be preſented in my name to M. Bonnet, who, on the tenth of Auguſt, wrote to me; 'All the ſnails arrived in the beſt ſtate; and have 'ſhewn their admirable reproductions better than

'thoſe

'thoſe on which I made my experiments. Your's 'are of a ſpecies with which I was unacquainted; 'they ſeem in ſome reſpect to approach the 'large ſpecies of our country, but differing in co-'lour and the ſmallneſs of ſize. Reproduction 'is evidently more eaſily accompliſhed in your's 'than in theſe, or in the French ſpecies, which I 'conclude both from my own experiments and 'thoſe of others.' And he adds, 'Your ſnails 'have given the beſt evidence of reproduction, 'by gnawing through the paper that confined 'them.'

From my experiments, I was conſcious I had not been deceived, whatever the authors in the preceding ſection might oppoſe; conſidering alſo, that their experiments were negative, and mine poſitive. The logic teaching a poſitive fact is not overturned by a thouſand negatives; becauſe the accidents are innumerable, and moſt of them unforeſeen, that may diſturb the ſucceſs of an event. Yet, notwithſtanding all theſe reaſons, had my own experiments only, argued in my favour, I ſhould not have been completely ſatisfied. But, beſides M. Bonnet, the reſt of my learned friends, who, with their own eyes had beheld this phyſical fact and with admiration, were perfectly convinced, ſtill I had reaſon to doubt whether the ſame would be admitted by an enlightened public; and every author, who publiſhes his diſ-

coveries,

coveries, ſhould be very earneſt that this will be the caſe. Thus, the readers who, after peruſing my experiments, proceeded to thoſe of my opponents, found themſelves between two contradictory authorities; and, notwithſtanding the confidence with which I had made a poſitive aſſertion in the affirmative, this was inſufficient for diſſipating every ſhadow of doubt or ſuſpicion from their minds, which might be excited by the unheard of ſingularity of the fact, and perhaps confirmed and augmented by ſuch a number of other facts tending to impugn it. There was no method more effectual towards conviction, than that this diſputed reproduction ſhould come under the rigorous inveſtigation of able naturaliſts; and that they ſhould aſcertain it in ſuch a manner as to leave no room for contradiction. This has happened. Beſides the internal complacency which every philoſophic enquirer feels in finding his own experiments confirmed, I alſo have the ſatisfaction to obſerve that moſt of theſe naturaliſts have been pleaſed to take my part, without the leaſt relation exiſting between them and me, or the ſmalleſt literary intercourſe. Therefore I judge it of great importance to my cauſe, which is that of truth, to collect together the facts that favour it.

In

In the *Avant Coureur* are alſo ſome experiments corroborating mine; the firſt of which is this.

'H. Roos, a German, now reſiding in Paris, 'has undertaken to verify Sig. Spallanzani's ex-'periments on decapitated ſnails. Some died; 'others ſurvived the operation. One reproduc-'ed the larger horns; another, which had been 'decollated at the root of the horns, acquired a 'new head and four new horns,' *Journal*, No. 30, 35, 47, 1768.

This experiment correſponds with thoſe of M. Lavoiſier, related in the ſame journal, No. 38. 'We now ſpeak of Sig. Spallanzani's ſingular 'diſcovery, concerning the reproduction of the 'head of the ſnail, with more confidence, becauſe 'M. Lavoiſier has ſhewn the Royal Academy 'of Sciences a ſnail which he decapitated on the '12 of laſt June, which has reproduced a new 'head exactly ſimilar to the firſt. He has ſeve-'ral more, with the reproduction leſs advanced. 'The head was cut off a little beyond the large 'horns. More than a month elapſed before there 'was any ſymptom of regeneration, which a 'ſmall papilla or tubercle announced. The horns 'are much thicker than in their natural ſtate, 'and only a line and a half long. M. Lavoiſier 'has remarked, that the part called the tail is alſo 'capable of reproduction.'

But

But M. Lavoiſier is not the only academician who has ſeen the reproduction, as we learn from the memoirs of the Royal Academy. 'Since Sig. 'Spallanzani's diſcovery, many ſnails have been 'decapitated. From the obſervations communi- 'cated by Meſſrs Turgot, Tenon, Heriſſant, and 'other philoſophers, the head was completely ſe- 'vered from ſome ſnails; of others, longitudi- 'nally ſplit up, and one half taken away; and 'ſome had the horns cut off, or pulled out.

'In about a month, a protuberance forms in 'the middle of the ſection, which gradually 'grows, and at laſt becomes a new head, pro- 'vided with its mouth and teeth. M. Heriſſant 'has demonſtrated that theſe teeth are reproduc- 'ed, having ſhewn a ſevered head, with the teeth, 'preſerved in the ſpirit of wine, though there 'were alſo teeth in the regenerated head of the 'ſame ſnail.'

'The horns are not reproduced until the head 'is entirely formed; and their growth preſerves 'no uniform rule.'

Theſe are all the experiments, ſo far as I know, made in Paris. And I have next to ſpeak of thoſe undertaken by other naturaliſts, in different parts of Europe: and, as I am deſirous of continuing the ſame chronological order which I have hitherto endeavoured to preſerve, we ſhall, in the firſt place, advert to a treatiſe by Muller

of

of Copenhagen, printed 1769, and republiſhed in his work, *Vermium terreſtrium et fluviatilium ſuccincta hiſtoria*. And as this treatiſe is not very ſhort, I ſhall only give an abridgement of it.

1. The German naturaliſt accompliſhed decapitation with very fine ſciſſars, when the ſnail was fully ſtretched; taking the greateſt care not to cut off the part uſually called the foot. 2. Immediately after decollation, he conſtantly examined the amputated head, both with his naked eye and a magnifier, and alſo ſhewed it to learned men, who beheld not only the four horns, the extremities of the two larger with eyes, but the mouth, lips, and jaws. 3. Though part of the ſkin and horns may be taken away, and not the head, an exact obſerver cannot be deceived, by examining the ſevered portion which remains on the blade of the ſciſſars; and he eaſily judges whether or not the operation has been properly performed. 4. The reproduced parts are diſtinguiſhed from the reſt by greater whiteneſs and tranſparency; while thoſe ſeparated always continue obſcure. 5. Many accidents evitable and inevitable may impede reproduction; but a ſingle operation, accurately performed, will prove the fact without reply.

Theſe are preliminary remarks; and the ſum of Muller's obſervations is as follows.

Both

Both the *pomatia* and *nemoralis* he found inept for reproduction, but it was otherwise with a variety of the latter. On the 19 of July 1768, one was decapitated, and part of the neck, along with the anterior portion of the foot, likewise cut off. It was long before reproduction commenced. The foot, however, was repaired on the 16 of September; but the head did not appear before spring 1769: and in summer, the whole was complete, except the mouth, lips, and smaller horns.

In another snail, decollated 14 September 1768, the upper lip was already formed, March 1769, and the opening of the mouth had become visible. A third, decapitated 14 September, had repaired most part of the head during the following summer.

Muller terminates his memoir with observing, that a subsequent journey had prevented further examination of the reproducing heads; but that the restitution of separated parts had been indisputably proved.

The Padré Scarella of Brescia repeated my experiments in the same year, 1769, and communicated the result in the following letter: 'After 'four months silence, I am now to relate what it 'has been in my power to observe respecting 'snails, and also the observations of a skilful ana-'tomist, my friend Sig. Ludovico Pusini, who, 'I hope, will in time publish his anatomical and 'physiological

'physiological discoveries. On the 27 of April, 'in presence of many professors of medicine and 'philosophy, I decapitated sixty-two snails a little 'below the large horns. Until the end of last 'month, they remained undisturbed in a box, 'exposed to the air of the window in a close 'apartment; only, from time to time, I threw 'away those which were evidently dead from the 'fœtor. Since the tenth of September, I have 'found twenty-two alive, which protruded the 'head partially, or entirely reproduced when sti-'mulated. And this was clearly recognised by 'every person present. The head of one was 'completely repaired; another had the two 'large horns; others, part both of the large and 'small; and, in all the rest, the progress of re-'production was various. Sig. Pusini shewed 'me a snail with the head completely repaired.

'I am, with great esteem, your most humble 'and devoted servant, GIAMBATTISTA SCA-'RELLA. Brescia, 28 September 1769.'

The year after this gentleman politely communicated his observations, I understood, from my illustrious friend M. Bonnet of Geneva, that M. Schaeffer of Ratisbon had lately published an account of his experiments, which confirmed mine in a surprising manner. Being very earnest to obtain further information, I resolved to address the author himself; from whom I had this obliging

obliging letter: 'It has so happened, Signor, 'my experiments and observations on snails ful- 'ly verify your's. Perhaps my curiosity has been 'extended a little farther: but I regret that the 'dissertations are written in German, and that I 'have not had leisure to make an abridgement 'for you in Latin or French. This defect can 'only be supplied by subjoining a brief extract 'which a friend has composed. And I remain 'your most humble and obliged servant, GIA- 'COMO SCHAEFFER. Ratisbon, 8 March 1770.'

Extract concerning the reproduction of snails.

'In the year 1753, M. Ziegenbalg, a learn- 'ed Dane, presented a memoir to the academy 'of Copenhagen, where he communicated that 'some snails that had been decapitated, were still 'alive, and continued to come out and retire in- 'to their shells as usual. Although this must 'have appeared a very extraordinary phenome- 'non, it does not seem to have met with the atten- 'tion it deserved. Nor was it until March 1768, 'that Father Boscovich announced to M. de la 'Condamine, that the Abbé Spallanzani had 'decapitated several snails; and not only did they 'live, but, after retiring a certain time into their 'shells, came out as they do naturally, and at last 'had regenerated a new head organised like the 'first.

'Observations

'Obſervations of this deſcription could not fail 'to attract the attention of naturaliſts: and the 'celebrated Schæffer of Ratiſbon, in particular, 'has put this phenomenon beyond all diſpute. 'That eminent naturaliſt, having frequently re- 'peated experiments on ſnails, had the head and 'alſo the tail reproduced. Laſt year he publiſh- 'ed his experiments in a tract illuſtrated with co- 'loured figures.

'They have lately been repeated. On the 'ninth of May, twenty-ſix ſnails were deca- 'pitated: Only two died from the operation; all 'the reſt lived, and regenerated the head.'

In the year 1769, Sig. Ab. Troilo, librarian to his Serene Highneſs the Duke of Modena, and Emeritus Profeſſor of Experimental Philoſophy in the ſame univerſity, engaged in the ſubject, and next year communicated his reſults to me.

On the fifth of May, one hundred and twenty-four ſnails were mutilated. The whole head, and a portion of the neck were cut off 68. From 28, the head and no more. From other 28, one half of it, that is, the portion including the two ſmaller horns, the lips, mandibles, tooth, and the various muſcles appertaining to theſe parts.

Forty-nine of the firſt diviſion had periſhed againſt the twenty-ninth: but one began to repair a portion of the head: ſome others had regenerated the left larger horn, and all the reſt,

except one, exhibited a principle of reproduction.

More snails died in June; and on the 14, of all the sixty-eight deprived of the head and part of the neck, not one remained alive.

With the 28 decapitated, without injuring the other parts, it was very different. Only one had died twenty-four days after decapitation: and the originating horns of almost the whole were budding; or, indeed, the rudiments of the head; and the regeneration of some was complete in the beginning of June, for they ate lettuce presented to them.

It was much more credible, that reproduction of half the head would be easier obtained. The Abbé terminates his letter, remarking that, notwithstanding the facility of reproduction, the head of some snails was not entirely repaired during the first days of September; and of the monstrosities attending the various generations, one was very singular: it consisted of two horns of unequal length, situated on the right side of the head, which, after being always united, and as if incorporated together, came at length to form a single horn thicker and shorter than a large one is naturally.

Several years after these experiments, and when I was thinking little about them, three new treatises were published confirming my observations,

tions, two by M. Bonnet, and the third by M. Senebier, librarian to the Republic of Geneva. The chief motives inducing both theſe naturaliſts to experiment on ſnails were the doubts ſtarted by Meſſrs Adanſon and Murray againſt reproductions. M. Senebier's account of his experiments is in a letter to the Abbé Rozier, and inſerted in the Journal de Phyſique. I ſhall tranſcribe the whole as it is very ſhort.

'I avail myſelf of this opportunity to announce, that I have repeated the celebrated 'Abbé Spallanzani's ſingular experiments on the 'regeneration of the head of decollated ſnails. I 'have lately peruſed the Gottingen Gazette, 'where there is an extract from a memoir by 'Profeſſor Murray, which puts the matter in 'doubt: and becauſe I know the truth of theſe 'obſervations has been queſtioned by no contemptible obſervers both in France and Italy, I 'now write you this, as the ſeaſon is favourable 'for reproduction, and fit for repetition of the 'experiment.

'I decapitated twelve garden ſnails, 15 April, 'and aſcertained that ſeparation was complete, by 'inſpecting the ſevered head with a magnifier, 'and comparing it with Swammerdam's exact deſcription. In three weeks, one exhibited the 'head and larger horns reproduced: the ſmall

'ones alſo budded, and the ſnail was ſoon as 'perfect as before decapitation.'

M. Bonnet, by numerous and correct experiments, eſtabliſhed the truth of mine (1).

Here terminates the reſults by which Meſſrs Roos, Lavoiſier, Turgot, Tenon, Heriſſant, Scarella, Schæffer, Troilo, Senebier, and Bonnet have confirmed my diſcovery. But this diſcovery, I hope, will acquire greater confidence among the learned, from being enriched by three inedited treatiſes; the publication of which I am certain will be moſt agreeable to the reader, as they are the production of three able Italian profeſſors of anatomy, already celebrated by their intereſting works: and theſe are, Sig. Caldani, Girardi, and Pratolongo, junior. I lately requeſted theſe gentlemen to make experiments on our reptiles, and with philoſophic candour to inform me of their ſucceſs. They complied; and the following letters are tranſcribed in the order of time that I received them.

'Moſt eſteemed Friend,—Behold me at laſt 'prepared to ſatisfy your requeſt, by relat-'ing the ſucceſs of my obſervations on the re-'productive faculty of ſnails. The ſimple recital 'of facts will be ſufficient; for the journal of ob-'ſervations

(1) I conceive it ſuperfluous to give any extract from his Memoirs, as they immediately follow this.—T.

'ſervations is too long, and ſo ill written, that it 'would give you conſiderable trouble to read 'it: nor have I time to make a correct co-'py.

'My experiments began laſt July; and I uſed 'the ſnails commonly found in gardens, of a 'figure approaching to ſpherical, and with a 'dark brown ſhell. Seventeen, ſome large and 'ſome ſmall, were decapitated behind the large 'horns. Decollation was performed on a ſmooth 'wooden tablet, with a ſtrong ſharp razor. The 'ſnails inſtantly retired into their ſhells: a viſcous 'frothy matter exuded from the trunk, in many 'of a greeniſh colour. They were immediately 'put into a veſſel among ſoft earth, and covered 'with a ſieve.

'Next I proceeded to diſſect the heads, after be-'ing kept a little in warm water; and in all, found 'the upper and under part, with the horns, 'mouth, lips, teeth, and a portion of the pha-'rynx. I know not whether I ſhould inform 'you, that theſe ſnails were what the French call '*eſcargot*, and are deſcribed by Swammerdam in 'his *Biblia Naturæ*. The general appearance 'ſeemed to correſpond with this; but, aſſiſted 'with a good magnifier, ſome difference was vi-'ſible, as Swammerdam's are in various places 'covered with ſmall iſolated tubercles of an irre-'gular figure, whereas mine were wholly cover-

'ed with longiſh tubercles cloſely united, and 'like ſo many elliptical pellucid veſicles, almoſt 'all of equal ſize, but not iſolated, and they 'were ſurrounded by another ſubſtance evident 'to the eye. When the animal's body was ex-'tended, the veſicles were long, when contrac-'ted, they became round.

'From this imperfect deſcription, ſo able a 'naturaliſt as you, will recogniſe the ſnails which 'were the ſubject of my experiments. Perhaps 'the French word *eſcargot* means ſnails in gene-'ral, and no particular ſpecies.

'Only four of the ſeventeen ſnails ſurvived. 'Whether this unlucky circumſtance was owing 'to the exceſſive heat of the weather, or my too 'impatient curioſity, not conſidering how the 'wound might be affected by the air, I cannot 'well determine. Two died in four days; a 'third on the twenty-eighth, other two on the 'thirty-firſt day; and three more lived thirty 'three. Then I did not find one dead until the 'ſixty-ſixth day, and the reſt on the eighty-'fourth.

'All had exhibited ſome marks of reproduc-'tion; two had the large horns half regenerated, 'the lips and teeth alſo made great progreſs. 'There was a monſtroſity in the horns as they 'were united. In the other eleven, a tubercle 'roſe from the trunk, and in the centre of it a

'black

'black point, which included the horns joined 'in one. In other five, that repaired the head 'completely, were two black points on the ſides 'of the tubercle, and a black line was afterwards 'obſerved connected with them. Theſe points 'were manifeſtly the eyes and the lines, the op- 'tic nerves. One of the heads was not repaired 'in leſs than four months and a half. The re- 'production was undoubted, becauſe all four, 'which are ſtill alive, fed on young lettuce, and 'alſo drew the operculum within the ſhell.

'There is no wonder that nature has provided 'a protection ſuch as the cover; for decapitated 'ſnails, from long abſtinence, want that gluten 'with which their loſs is repaired; and it is al- 'ways thinner, according to the duration of ab- 'ſtinence.

'The reparatory matter of the operculum fails 'in different periods of time. Some ſnails ceaſe 'to repair it in nineteen days, others in forty- 'eight. The defect of gluten alarmed me for 'the ſafety of my four remaining ſnails, there- 'fore I kept them in a warm ſituation; and they 'repaired a very thin operculum, which was 'proof they had fed.

'The reparation of the ſmall horns is always 'more tardy, compared to the large ones and the 'lips; neither are the horns always of the ſame 'length and thickneſs. I have never ſeen the

'ſhagreen ſkin repaired. Your's, CALDANI. Pa-
'dua, 27th December 1783.'

Next is a letter from Sig. Pratolongo.

'By your Reverence's moſt obliging letter, I
'underſtand that you deſire ſome information
'concerning my obſervations on the reproduc-
'tion of ſnails. This I conſider an expreſs com-
'mand, which I the more willingly obey, as I
'have the pleaſure of numbering myſelf among
'thoſe who have confirmed your diſcovery.

'My experiments were made in 1780, in con-
'ſequence of ſeveral anonymous letters in jour-
'nals publiſhed here, where I was defied to ſhow
'the reproduction of the head of ſnails, decapi-
'tated, when I firſt came to this city. Being
'unable to ſatisfy their curioſity, becauſe all theſe
'reptiles had periſhed, from the bran, I believe,
'where I had incautiouſly put them, entirely co-
'vering them; and reflecting that it would be to
'no purpoſe, referring to the ſucceſsful experi-
'ments of Muller Roos, Schaeffer, Bonnet, La-
'voiſier, and other moſt accurate naturaliſts,
'who confirmed this diſcovery, I reſolved to
'repeat my experiments.

'On the 10 of July, the time reputed moſt fa-
'vourable for reproduction, I took twelve ſnails
'of the ſpecies called Pomatia by Linnæus in
'his *Fauna Suecica*, and ſimilar to thoſe which I
'ſent your Reverence laſt year. Anxious that my

'experiments

'experiments ſhould ſucceed, I uſed all neceſ-
'ſary precaution; for, by neglecting it, theſe rep-
'tiles periſh, as muſt have happened to thoſe of
'M. Adanſon, Bomare, and Father Cotté. But
'I did not follow the plan adopted by others of
'immerſion in water to force them out. Had
'M. Bonnet omitted it, his ſnails would have ſur-
'vived. Perhaps in this I might be too ſcrupu-
'lous. My method was to put them on young
'graſs, and wait patiently till they were ful-
'ly extended, then, with a pair of ſharp ſciſſars, I
'ſevered the head immediately behind the large
'horns. Each ſeparated part was examined both
'by myſelf and thoſe aſſiſting at the operations.
'The ſnails were then ſhut up in a jar, covered
'with a paper full of holes, that the internal air
'might change, and not become prejudicial to
'them.

'On the 28 of Auguſt, I found the paper torn,
'and ſome of the ſnails ſticking to the outſide of
'the veſſel. They all appeared when put on
'graſs, but reproduction had made unequal pro-
'greſs, for only a tubercle appeared on the trunk
'of ſome, while in others reproduction was com-
'plete.

'I ſent many of the ſnails to the editor of the
'journal, with a letter, in which I requeſted him
'to invite our critics to examine my proofs. In
'the next journal, I had the ſatisfaction to ſee an

'anſwer,

'anſwer, where they acknowledged themſelves 'convinced that it was a phyſical truth, which, 'miſled by ill made experiments, they conſidered 'a prodigy not well aſcertained.

'I ſhould not have thought of decapitating 'more of theſe reptiles, had not the progreſs of 'reproduction in ſeveral, and in the order of va-'rious parts, ſeemed ſtrange, and this induced 'me to inveſtigate the principles on which ſuch 'varieties depend. The conſtancy preſerved, as 'I know, from comparing my experiments with 'thoſe of your Reverence, M. Bonnet and Se-'nebier, convinced me that they were not *luſus* '*naturæ*, but depending on fixed and invariable 'laws. But were theſe laws regulated by the 'ſite of the cut, or by its greater or leſs obliqui-'ty? This is one of the queſtions which occurs 'in your Prodromo.

'In the end of laſt February, I decapitated 'twelve of the ſame ſnails as before: but there 'was a conſiderable difference in the nature of 'the cut. In one half, it was vertical, but a 'little farther from the large horns than for-'merly; in the other half of the number, it was 'made more or leſs oblique, leaving one large 'horn of ſome untouched. All were then con-'fined in a veſſel. Having examined them in a 'month, I found five dead; however, the other 'ſeven had formed their operculum. On theſe,

'therefore,

' therefore, my experiments were to be made: ' but, recollecting Sig. Plateretti's obſervation, ' that violence done the animals for this purpoſe ' is fatal, and frequently makes the experiment ' fail, I delayed until July, and then broke the ' operculum; and, having put them on graſs, I ' found the whole alive, but the reproduction un- ' equally advanced as in the firſt. Only two with ' the vertical cut ſurvived; one horn was renewed ' on a whitiſh globular ſubſtance, projecting from ' the trunk of one; and the head of the other ' ſeemed to be completely regenerated. The ' two large horns were of different lengths. In ' the five cut obliquely, reproduction was a ſhape- ' leſs lump, which had not yet acquired its pro- ' per configuration. The horns of ſome were ' regenerated, and in various ſtates of advance- ' ment; while, in others, they did not even bud.

' As I remarked the ſame variety in theſe ex- ' periments that occurred in the firſt, when the ' ſections were ſimilar, I inclined to think the ' inequality of the productive power, acting dif- ' ferently on different individuals, as well as its ' inequality of action on different parts of the ' head, were independent of the cut. But reflect- ' ing that the laws of ſuch phenomena ought not ' to be determined by few, but by numerous ' and repeated obſervations, I conceived it better

' to

'to suspend my judgment, and to multiply the 'experiments.

'Towards the end of last summer, along with 'Sig. D. Mongiardino, I decapitated a great 'number of snails, diversifying the cut in many 'different ways. It was vertical; one line, or 'farther, behind the large horns; more or less 'oblique; a line, or further, before the large 'horns; and only half the head of others was 'cut off. Ten died, and the rest, examined 'within these some days, were alive. But the 'reproductive power has little activity during this 'cold weather; and it will probably be a long 'time before I am able to satisfy my curiosity.

'With the most profound respect, I am your 'Reverence's most humble and obliged servant, 'Gio. Battista Pratolongo. Genoa, 10 'January 1783.'

To Sig. the Abbé Lazaro Spallanzani, royal Professor in the university of Pavia, Michele Girardi.

'My dear Friend,—I consider your request, to 'repeat the experiments you have made on ter-'restrial snails, as an evidence of that impartial 'anxiety which actuates you for discovering the 'truth amidst the mysterious arcana of nature. 'Thus your observations will always triumph; 'for they are supported on such a basis as to 'dread neither the injury of time nor envy. It

'seems,

'seems, by a fatal deſtiny, that the luminous and 'uſeful diſcoveries of every period have been 'doomed to meet with bitter opponents, who, 'though unable to oppoſe thoſe beneficent rays 'diffuſing around and diſpelling darkneſs, yet 'have ſometimes tended to retard the advance-'ment of ſcience. If I am not miſtaken, this ob-'ſtruction to human knowledge ſprings from 'two ſources; from preſumption, which is com-'monly the child of envy, and from pride.— 'From preſumption; becauſe ſome, who diſdain 'obſervation, know not, and cannot perſuade 'themſelves that the things which ſurpaſs the 'bounds of their limited underſtanding are true: 'hence, unacquainted with what is wonderful and 'uncommon in the admirable works of nature, 'they deny all which they are ignorant of. '—From envy, on the other hand, becauſe there 'are certain deſpiteful men, who will ſit on their 'chairs, and, conceiving themſelves very learned, 'can ill abide that others know more than them-'ſelves: then erecting a tribunal, and, incapable 'of doing any thing elſe, they imperiouſly con-'demn whatever oppoſes them, or interrupts 'their progreſs to the ſummit of that glory, which 'they imagine themſelves alone entitled to en-'joy.

'But among theſe, I certainly cannot number 'M. Adanſon, Wartel, Cotté, and ſo many 'more,

'more, and, in particular, Mr Adolphus Murray, 'a learned and candid man, and my particular 'friend. Although I do believe, that they con- 'troverted your diſcovery, from not having re- 'peated the experiments with accuracy, ſtill it is 'more rational to ſuppoſe they have been made 'on ſnails which want the reproductive faculty, 'as we have often obſerved. Such are the per- 'ſons that alone merit an account of this ſingular 'and admirable reproduction, ſince it would be 'vain to addreſs others, who neither can nor will 'be perſuaded.

'Before tranſcribing the reſult of my obſerva- 'tions, permit me to obſerve, that I have ana- 'tomiſed ſnails as much as poſſible, and eſpeci- 'ally the head, which is much more compound 'than any one could ſuppoſe. But it is extremely 'difficult to accompliſh this, while alive, as, who- 'ever takes the trouble of examination, will eaſily 'find. It is equally difficult to make obſerva- 'tions on them in their own proper habitations, 'for they are there ſo much ſhrunk and contract- 'ed within themſelves: and, if the obſerver en- 'deavours to examine them, when ſtretched out of 'the ſhell, they contract and retire ſo quickly, that 'the anterior part of the foot is inſtantly turned 'out, and the head concealed as if in the centre of 'the body. And the difficulty ſtill augments, if 'we add that tenacious viſcous matter copiouſly 'diffuſed,

'diffuſed, and the great muſcular power, by 'means of which they contract more forcibly, 'whenever touched by the knife.

'To facilitate every thing as much as poſſible, 'in my diſſections, I practiſed three methods. 'One propoſed by the celebrated Swammerdam, 'to whom we owe ſo much on the ſubject, was, 'by allowing the ſnails to die in water; another, 'by taking away the ſhell, and thus leaving them 'to periſh; and a third, by putting them in cold 'water, and then boiling it a little. By theſe 'different methods, they frequently remain with 'the neck and head, and ſometimes alſo the 'horns, ſtretched out, in this manner affording 'me opportunities for convenient examination.

'When a ſnail extended, and carrying along 'its habitation, is viewed, or when dead, in a ſi- 'milar poſition, a long neck, terminating in the 'head, appears. Two larger and two ſmaller 'horns riſe from the head. A minute globe is 'at the extremity of each; and in the two larger 'is a black point, called the eye. Around the 'poſterior part of the neck is a circular promi- 'nence, called the lap or collar; on the right of 'which appears a hole evidently for reſpiration; 'and in it the inteſtine for diſcharging the fæces 'alſo terminates. A glandular ſhagreen ſkin, 'variouſly coloured according to the different 'ſpecies, covers the upper part of the neck and 'tail;

'tail; and an extremely thin membrane covers
'the under part, with which the ſnail advances,
'and is called the foot. In the middle are lon-
'gitudinal lines, and on the ſides tranſverſe ones,
'indicating the tendons of the muſcles below,
'ſerving for action and re-action. This anterior
'and extreme part of the foot contracts with the
'lower mandible, leaving a little vacuity, pro-
'bably that the motions of the mandible may be
'more free and quick. At the root of the ſmall
'horns are the lips; and when theſe are opened,
'the teeth, tongue, and cavity of the mouth, are
'ſeen. Under the right large horn, and perhaps
'a little behind and near the foot, is obſerved a
'ſmall white mark amidſt the tubercles of the
'ſkin, which is the female vagina. And, as all
'ſnails are hermaphrodites, the male apparatus
'for generation alſo proceeds from the left ſide,
'plate 7, fig. 1, b. This mark, which even the
'moſt practiſed obſervers can with difficulty diſ-
'cover, becomes turgid at the ſeaſon of their
'amours. More than once I have had the good
'fortune to ſurpriſe them in copulation, and ſee
'their mode of proceeding. After they approach
'each other, it is entertaining to behold how
'they mutually examine and re-examine with
'the head and neck, before uniting, and ſeem
'as if by turns to excite each other and invite to
'copulation. Then, having diſpoſed themſelves
'for amorous intercourſe, the male apparatus,

'turgid

'turgid with cerulean blood, is protruded from 'the head, and mutually introduced into the fe- 'male aperture, penetrating deep within; and 'they are entwined in such a manner, for two 'hours, or even more sometimes, that they will 'sooner suffer the rupture of the male organs 'than desist from copulation. By a forced se- 'paration, it is easy to observe how the canals, 'from which the male organs issue, surround 'and embrace the origin of the appendage to the 'uterus or vagina, and, in that situation, how 'much they are distended with a cerulean blood.

'All this may be viewed without the aid of 'the scalpel; but it becomes necessary, when we 'desire to examine the internal organization, and 'particularly that of the head, which is the chief 'object of our research. On longitudinally cut- 'ting and folding down half the skin which co- 'vers the neck, head, and cutaneous muscle, 'there is, in the first place, observed, besides a 'fine arachnoid membrane on the extreme an- 'terior part, a prominent ovular globe, compre- 'hending the two mandibles, the mouth, tongue, 'and also the origin of the œsophagus, plate 7, 'fig. 1. b b. The upper mandible is cartilagi- 'nous, and forms the palate within: from the 'higher part a semilunar osseous tooth projects, 'which in colour and shape resembles one of the 'tortoise shell combs wore by our ladies for or-

'nament on their hair. The tooth is an aggre-
'gate of ſix or ſeven *dentes inciſores*, pointed be-
'low, but ſo united as to form only a ſingle
'tooth. The jaw, or rather gum or under lip,
'has no teeth; it is ſeparate from two ſoft ſub-
'ſtances of a dark colour towards the œſopha-
'gus, which confine the tongue. The mouth is
'bounded before and behind by the teeth and
'œſophagus; above and below by the palate
'and tongue.

'The tongue is not free; it is bridled and
'united to the under jaw: two furrowed carti-
'laginous membranes form its ſubſtance; the
'upper connected with that which covers the
'under jaw, and, curving behind, deſcends with
'an inflexion, and comes forward under the
'lower membrane; then receding, it terminates
'in a ſmall dark globular appendage, which pro-
'jects from the under part of the ovular globe,
'fig. 2. d. This appendage contains a white
'longiſh curvated maſs, which, being moveable
'and connected with the anterior part of the
'tongue, where the œſophagus commences, does
'not ſeem to be placed there by nature without
'ſome particular uſe. The lower membrane of
'the tongue, which is much thicker than the
'upper, convex above, ſemicircular and hollow
'below; and is rooted in the fore part of the
'under jaw, and terminates free where the upper
'membrane

'membrane is inflected behind. This peculiar 'ſtructure of the tongue ſeems to ſupply the de- 'fect of teeth; whence maſtication is performed 'by the action and reſiſtance between the palate 'and tongue.

'Where the mouth and palate terminate be- 'hind, the œſophagus commences in the higher 'and poſterior part of the ovular globe, and be- 'tween the two ſalival ducts, which unite to the 'globe by penetrating the cavity of the mouth. 'It is very narrow at the origin, but dilates fur- 'ther down. It is of a light aſh colour, and 'formed of a thin membrane longitudinally cor- 'rugated, by which means it eaſily dilates and 'contracts.

'Above the anterior part of the globe is ſome- 'times found the brain, of a paliſh colour, and 'in a manner divided into two lobes, fig. 1, i. 'I ſay *ſometimes*, becauſe the brain of ſnails is 'moveable, and changes its place according to 'the motion of the animal: at times advancing 'before, and at times receding behind the ovular 'globe above the œſophagus, eſpecially when 'the ſnail ſtretches out of its ſhell. This is ef- 'fected by means of muſcles traverſing the ſides 'of the brain, and admirably united to it by ſe- 'veral connected interwoven filaments. The 'brain is but about a line in breadth, and not 'more than one and a half in length; larger,

'however, in proportion to the size of the snail. 'It comprehends numerous nerves; and al- 'though they have been accurately described by 'the celebrated Swammerdam, it does not seem 'totally unconnected with our inquiry to enu- 'merate them here: observing however, if there 'should be any trivial difference, that the fault is 'not to be ascribed to another, but I will rather 'be content that it is attributed to my inferior 'dexterity in examining objects so minute; and 'more particularly, as, in the course of this in- 'vestigation, thinking myself able to distinguish 'the objects without any hazard of error, I have 'used no magnifier to increase them above their 'natural size.

'Of the nerves that divaricate into the head, 'some proceed immediately from the brain, others 'by means of a ganglion formed of the lobes of 'the brain itself, which they invest according to 'its different position, sometimes the anterior 'portion of the ovular globe, and sometimes the 'œsophagus. They are little larger than a millet 'seed below and also behind the globe. Fol- 'lowing the order of dissection, we shall first 'speak of the nerves of the brain, and then of 'those of the ganglion.

'In the brain originate twelve nerves, that is, 'six on each side, which I shall call six pair: 'some run behind, and others before. The

'first,

'firſt, which we may denominate the muſcular,
'originating in the lobes of the brain, proceed
'back to inſert themſelves into the muſcle *retrac-*
'*tor* of the ovular globe; the other five are diſ-
'tributed before, and are the upper and under
'labial, the mandibular, the optic larger and
'ſmaller. The upper labial ariſe in the anterior
'part of the brain, and, traverſing the ſides of
'the oval, divide before into two conſpicuous
'filaments; one proceeds to the higher part of
'the upper lips, and the other terminates in the
'lower part of them. The mandibular nerves
'originate in the poſterior part of the brain, and,
'proceeding back, are inſerted in the ovular
'globe, in the vicinity of the ſalival ducts, di-
'verging into the mouth, throat, and palate.
'The optic nerves iſſue from the ſides of the
'brain, and, by means of either membrane, are
'attached to the muſcle *motor* of the larger horns,
'and terminate in a pyriform bulb at their ex-
'tremity. The ſmaller optics, or more properly
'nerves, of the ſmall horns alſo ariſe from the
'ſides of the brain, in the vicinity of the larger
'ones. They unite to the muſcle of the ſmaller
'horns; and, divided into ſeveral filaments, pro-
'ceed to terminate in the extremity of the horns,
'in the cutaneous muſcle and the extremity of
'the under jaw.

'The name *ſmaller optics*, which I give theſe 'nerves, may to ſome appear too arbitrary, and 'perhaps be attended with inconvenience, was 'not adopted from the property of the nerves 'themſelves, but from their affinity with thoſe of 'the large horns, which are commonly called 'optic nerves. They had this denomination 'ever ſince the celebrated Swammerdam, with 'the aid of powerful microſcopes, was able to 'find the organs of viſion in the black point 'projecting from the extremity, namely, the 'uvea, the three humours, and the chryſtal-'line lens inveſted by its membrane. I will not 'deny this admirable ſtructure, which it would 'not be eaſy for another to confirm; but my ob-'ſervations induce me to believe, that if the or-'gan of viſion is in theſe horns, that of touch is 'ſtill more certainly there: and this is the more 'evident, as the organ of touch is moſt ſenſible, 'while viſion is weak and confuſed, which gives 'very good reaſon for uncertainty. I made nu-'merous experiments with a view to remove the 'doubt, but they all concurred in increaſing it. 'When the horns were fully ſtretched, and the 'black points conſpicuous, I brought various 'ſubſtances cloſe to them, but they gave no in-'dication of certain and diſtinct viſion: becauſe 'I have ſeldom obſerved that ſnails either deviat-'ed from their route, or ſo much as bent the

large

'large horns to avoid the ſubſtances almoſt in 'contact with them. And what further added 'to my doubt was never being able, with the 'bright light of a candle, or concentrated rays of 'a lens, to make the horns alter their direction, 'unleſs when forced to retract by too intenſe 'heat. Although, by approximating a luminous or 'opaque body, they have afforded ſome ſymptom 'of viſion, repeated experiments, which are ad-'verſe to it, have confirmed my doubts, or at leaſt 'made me ſuſpect, if theſe do ſee, that their ſight 'is exceſſively obſcure and indiſtinct. But as 'their defect is very great in this, ſo are they 'moſt ſenſible in touch. Thus, when naturally 'ſtretched out, the horns are carried well extend-'ed before them, that they may be warned of the 'ſlighteſt obſtacle, juſt as a blind man is warn-'ed by his ſtaff to deviate from obſtructions in 'his way. It is ſufficient to behold ſnails advan-'cing to be convinced of this. They feel or 'grope along; no ſubſtance oppoſed ſtops their 'progreſs, but the horns unexpectedly ſtrike 'againſt it, and then are ſuddenly retracted: 'Next, being extended anew, they ſeem, by 'touching and retouching, to renew the exami-'nation; and, aſſuming the touch as the rule of 'direction after conſidering the obſtacle, deter-'mine whether to deviate from their route, if it is 'great, or to continue if it is inconſiderable. If

'the fact be such as it appears, it may reasonably 'be inferred, that snails have no eyes, or, if they 'have, nature has formed them of minute and 'almost unperceptible parts; whence objects are 'seen obscurely and confused: but in compensa-'tion for the dullness of this sense substituting 'the exquisiteness of touch, by which they can 'distinguish these same objects. Therefore the 'use of the globe on both large and small horns 'may be easily comprehended: namely, that the 'points of contact may increase by the surface 'being enlarged; and the exquisiteness of touch 'thus rendered greater for the use and defence 'of snails. But this is only a simple conjecture 'of mine, started for my own information among 'many others which you will observe.

'Let us return to the organization of the 'brain. We have seen that its lobes compose the 'ganglion, as the annular protuberance is form-'ed by the lobes of the human brain. Many 'nerves proceed from this ganglion, some for-'wards, others laterally; some backwards; and, 'lastly, others below. We shall treat only of 'those proceeding forward on the head, as parti-'cularly belonging to our subject. From the an-'terior part of the middle proceeds a nerve that 'advances in a straight line to insert itself in the 'posterior and under part of the oval globe. 'From the sides issue two bundles of nerves, one

'from

'from each, which ſpread on the cutaneous 'muſcle in many directions; ſome advancing to 'the root of the large horns; and in the right 'ſide are alſo inſerted in the prepuce and vagi-'na. From numerous ramifications of the lower 'nerves, ſome proceed forward, and are loſt in 'the anterior extremity of the foot.

'In the laſt place are the muſcles of the head, 'conſiſting of the retractor of the oval globe, the 'two retractors of the large horns, and two 'mandibles, which all iſſue from the muſcles 'that proceed from the ſpiral column. The re-'tractor of the globe, which is the largeſt of all, 'proceeds to inſert itſelf in the poſterior part of 'the globe below. Thoſe of the large horns are 'tinged with dark yellow before, and end in a 'ſmall oblong flattened hollow body, that ter-'minates towards the extremity of the large 'horns. The ſmall retractors paſs by the ſides 'of the brain, to which they adhere by means of 'the optic nerves; and thence dividing in two 'towards the extremities, the ſmaller paſſes to the 'extremity of the ſmaller horns, and the larger 'diſperſes in the muſcle of the ſkin below. The 'mandibular nerves, aſcending in ſeveral different 'and diſtinct bundles mutually inclined towards 'each other, proceed to unite at the apex of the 'under mandible, being equally diſtributed in the 'cutaneous muſcle.

'Beſides

'Beſides theſe are others which, although 'ſmall, and reſembling filaments, are very ele-'gant, and deſerve to be diſtinguiſhed. Theſe, 'which I ſhall name *fræna*, are ſeen in the upper 'and under part of the oval globe. The upper 'bridles are ſimple, Fig. 1. c. but the under 'triple. The former are on the ſides of the 'globe, in the vicinity of the ſalival ducts, and 'terminate in the upper lip. The under fræna 'are diviſible into large, ſmaller, and ſmalleſt, or 'long, ſhorter, and ſhorteſt. The long, Fig. 2. 'e e, originate under the ovular globe, where the 'muſcular retractor is inſerted, and terminate 'within the ſmall horns. The ſhort begin a little 'before the long, about the middle of the globe: 'thence diverging and joining the long, they ad-'vance to terminate in the right ſide of the ſame 'horns, Fig. 2. f. The ſmalleſt and ſhorteſt ori-'ginate before the ſhort, and terminate in a 'right line in the cutaneous muſcle of the un-'der jaw, Fig. 2. g. The contraction of theſe 'bridles draws back the lips, and brings for-'ward the globe; which action is ſo important, 'that it would be difficult to explain how, with-'out the bridles, the mandibles could be brought 'ſo much forward as to be on a level with the 'lips, which is particularly evident when the 'ſnail feeds, and eſpecially when endeavouring to 'gnaw a bit unſuitably hard.

'After

'Afer ſpeaking of the admirable ſtructure of 'the head, I ſhall proceed to the reſult of my 'laſt experiments on ſnails, ſuch as they have 'been ſince 1772 and 1773, with the ſame con- 'ſequences which you was the firſt to announce 'in your *Prodromo ſopra le reproduzioni animali*, 'and have ſo accurately deſcribed. The expe- 'riments I made were on ſome of the ſnails cal- 'led by the celebrated Linnæus, *Helices Poma- 'tiæ*, *Italæ*, *Luſitaniæ*, *Zonariæ*, *Arbuſtorum*, '*Nemorales Lucorum*, *Griſeæ*, &c. On the firſt 'of May 1782, I cut off the larger horns of 'twenty-four of thoſe which you gave me to car- 'ry from Reggio, and from an equal number, 'both the large and ſmall. From twenty-four, I 'cut the extremity of the tail, and as many were 'deprived of the head. A ſlice above two lines 'broad and ten long was alſo cut from the ſide 'of the foot of other twenty-four. Theſe various 'mutilations were repeated the ſame day on the 'like number of the ſnails of Parma, of the ſame 'ſpecies as the Reggian. The former were diſ- 'tinguiſhed by the letter A, and the latter by B; 'and all the reſt being alſo diſtinguiſhed, they 'were put into veſſels. The horns had been cut 'off with ſharp ſciſſars, while full ſtretched, ſome 'at the root, others through the middle. The 'head, tail, and foot were divided with a ſharp 'knife, when well extended from the ſhell on a

'hard

'hard plane, thus to be ſecure of the amputa-
'tion, and avoid the inconvenience often attend-
'ing it, more eſpecially when performed with
'ſciſſars (1). The head was ſevered about a line
'and a half behind the large horns.

'I have uniformly remarked a cerulean fluid
'come from the horns, particularly the larger,
'which bedews the dividing blade. This fluid is
'eaſily obſerved, if the operation is performed in
'water, and moſt perceptible if the water be il-
'luminated by the bright rays of the ſun. Not
'only do we then diſcover that the liquid is
'greeniſh, and is the blood flowing in the veſ-
'ſels of theſe animals, but alſo the quantity and
'force with which it ſpouts. I have ſometimes
'ſeen it continue ſeveral ſeconds, then ſtop, and
'again ſpring from the ſame wound in greater
'abundance. When the extremity of the horns
'is cut off, they frequently appear flaccid, faded,
'and fallen down on the lips, and their inability
'to ſwell and extend, ſeems to indicate, that
'one is not far from the truth who ſuppoſes,
'that to this fluid alone the protruſion and ex-
'tenſion of the horns are owing: becauſe, I
'have found nothing in all the external organi-
'zation, by my utmoſt exertions, I have never
'ſucceeded

(1) The ſame precaution has laudibly been obſerved by Dr Vincenzo Plateretti, as is mentioned in the *Scelta di Opuſcoli* di Milano.

'succeeded in finding either muſcle, tendon, or 'cartilage, deſtined for that office. The fluid 'iſſues in greater quantity on amputation of the 'head, in leſs, on mutilations of the tail or foot, 'and it is mixed with a viſcous tenacious matter. 'The ſeparated horns exhibit no ſigns of life, or 'they are very ſhort and feeble. It is not ſo with 'the head: for, ſix or ſeven minutes after am- 'putation, it diſplays unequivocal ſenſibility when 'irritated. Soon after decapitation, the ſnails 'contract and retire into the ſhell, but ſome re- 'turn in a little. In the place of amputation, now 'much contracted, a ſmall white mark appears; 'then they travel about as vivacious as if the 'head had not been cut off.

'The facility of motion and livelineſs of ſnails 'would perhaps induce one to think the head 'had not been properly ſevered, and this might 'in general be adopted as an indiſputable cer- 'tainty. But every doubt is diſpelled by the 'moſt ſcrupulous diſſection. In the ſeparated 'head, we behold the horns, jaws, teeth, tongue, 'and its appendages, all the nerves, muſcles, and 'bridles, the extremity of the œſophagus, the 'ſalival ducts, the prepuce, and alſo the vagina. 'Thus there is no queſtion that the complete 'head has been cut off.

'All the ſnails ſoon contract and retire within 'their dwellings. There they form that white

'tenacious

'tenacious operculum which is produced by 'viſcous matter that they generate. By this 'they are defended from external injury; but it 'is not more advantageous to them than detri-'mental to one deſirous of viewing them for ac-'curate obſervation. To do ſo, I have always 'endeavoured, as much as poſſible, not to aggra-'vate the effects of ſuch an immenſe wound as 'they have received, by any additional irritation, 'ſuch as breaking the ſhell behind, and ſome-'times, if obſtinate to appear, pricking them, 'which are not the actions of a maſterly hand, 'or of one accuſtomed to ſimilar experiments; 'and it is actually not uncommon, that thoſe 'ſtimulated in this manner die. But, to accom-'pliſh my object, I endeavour to entice them 'out, ſo that they may appear ſpontaneouſly, 'which has always ſucceeded on putting them in 'warm water, or expoſing them to the ſoft 'ſhowers of ſpring or ſummer; then they are na-'turally induced to come into the open air.

'In thoſe deprived of the large horns, or one 'half, I have never obſerved any thing new for 'ten, fifteen, even twenty days, nay, moſt com-'monly a month. In the place of amputation, 'a globule aroſe, which, becoming larger in time, 'exhibited a black point in the centre, exactly 'correſponding to that in the original horns. 'Two and more frequently three months have

'elapſed

'elapſed before both the large and ſmall horns 'were repaired.

'At firſt, the regenerated horns are of a pale 'white colour, and the ſkin is much more deli- 'cate than that of the head. They gradually 'acquire the proper hue, and form a whole ſo 'equal, that it is very difficult, even impoſſible, to 'diſtinguiſh the ſnails that have not loſt the 'horns. Diſſection proves that the renewed 'horns perfectly reſemble the firſt; they lengthen, 'enlarge, and are endowed with all the ſenſibili- 'ty of the firſt.

'In the ſame manner as the horns, are the tail 'and foot reproduced. I have remarked, that 'they regenerate with an equal continuation of 'the ſubſtance, and are ſooner completed than 'the horns. Theſe parts are alſo of pale white, 'which ſoon diſappears, and no traces of ampu- 'tation remain.

'What is obſerved in the horns, tail, and 'foot, is ſeen in the head, but with the following 'difference. In a month, or about that time, a 'globe projects from the centre, ſometimes it is 'towards one ſide, or, inſtead of a ſingle globe, 'two will appear, one on each ſide; the produc- 'tion of them is not always alike. Two, three, 'and often four months have been required for 'reproducing the head from one in the middle, 'and the ſame from two lateral globes uniting.

'But

'But from one lateral, I have almoſt uniformly 'ſeen an irregular production; for a large horn, 'ſurpaſſing the natural ſize, frequently grows 'from the extremity, and ſtill preſerves the pro- 'per organization. In my poſſeſſion, are ſeveral 'ſnails which have lived a year without this irre- 'gular production undergoing any change.

'The renovation of the head correſponds with 'that of the parts already deſcribed. At firſt, 'the ſkin is ſmooth and not ſhagreened; it is al- 'ſo of a lighter aſh colour, and thus diſtinguiſh- 'ed from the old ſkin, ſo that the new head ap- 'pears wonderfully applied to the original neck. 'Many of my friends beheld theſe reproductions 'with amazement; and you, along with the 'learned and celebrated Sig. Angelo Mazza, our 'common friend, when you was ſo kind as to 'viſit me laſt autumn. The difference of colour, 'which continues ſeveral months, afterwards 'changes, and there is no diſtinction between 'the new and old ſkin, but a faint furrow at the 'place of amputation, which alſo diſappears in 'time.

'Diſſection proves the exact reſemblance of 'the reproduced to the original heads, becauſe 'all the parts found in the one are found in the 'other; and what is more ſingular, although 'there is ſome difference between the colour of 'the neck and the head, all the internal parts are

'ſo

'so connected, equal and correspondent in co-
'lour, symmetry, and consistence, that it is im-
'possible to discriminate those reproduced.

'Whenever the head is finished, the snail uses
'the renovated parts to repair the long absti-
'nence it has been forced to undergo. During
'the beginning of October last year, I profited
'by a gentle shower to examine all the animals
'that issued from the shell. One of those peo-
'ple was with me who are naturally incredulous,
'and never less disposed to believe any thing
'than what borders on the marvellous. Not
'only was he obliged to acknowledge that the
'head was really reproduced, but he observed
'one endeavouring, with its prominent teeth, to
'gnaw a particle of bran, partly adhering to the
'shell, and in an inconvenient place. It was en-
'tertaining to see, in the snail's exertions, the re-
'peated action and re-action of the open mouth,
'the soft lips, tongue, and teeth, turning the par-
'ticle a thousand ways, until it effected separa-
'tion, and made itself a savoury mouthful.

'But this admirable reproduction does not
'succeed in all snails, and much less in those cor-
'responding to the great snails of Florence, nor
'ordinarily in the small ones called garden snails,
'and especially those found in orchards adorned
'with beautiful colours. My experiments were
'successful on the *helix pomatia*, *itala*, *zonaria*,
'*nemoralis*, *lucorum*. Of more than three hun-

'dred, which I have at different times decapitat-
'ed, ſome are ſtill alive without any regenera-
'tion, and ſome with an imperfect and irregular
'production, and the head of others is com-
'pletely renewed. I particularly remarked, that,
'in the greater part that died, decapitation had
'been improperly performed, or that it had been
'about four lines behind the large horns; con-
'ſequently a portion of the male organs, the ap-
'pendages to the uterus, or the uterus itſelf, had
'been cut off beſides the head. Perhaps ſome
'of thoſe that have not reproduced, or repro-
'duced imperfectly, are of too advanced age,
'and their fibres have loſt the neceſſary ſoftneſs,
'flexibility, and vigour; and, by induration, can-
'not extend to adopt the proper form for perfect
'reproduction.

'All that I have hitherto remarked happened
'equally to the ſnails of Reggio as thoſe of
'Parma, ſo that I have never obſerved any thing
'deſerving particular conſideration in the latter;
'which proves that ſnails of the ſame ſpecies
'univerſally afford the ſame reproductions, if ex-
'periments are correct.

'But it is time to terminate this long letter,
'which has undoubtedly exceeded the limits of
'your patience. Compriſing the whole, you will
'only ſee a confirmation of what you cautiouſly
'advanced in your Prodromo, namely, that ſnails
'poſſeſs the property of reproducing mutilated
'parts,

'parts, that is, the horns, the foot, tail, and 'head. When obſervations are accurate, and 'proceed from a philoſopher ſuch as you, who, 'in theſe matters,

'Siete maeſtro di color che ſanno,

'and uſing the requiſite precautions, they ſhould 'eaſily ſucceed every where, and moſt certainly 'do ſucceed.

'I ſend you theſe obſervations, not from being 'worthy to approach you: I know their inſigni-'ficance, and you alſo will be ſenſible of it; but 'becauſe you have deſired it, and that you may 'ſee how earneſt I am to give you ſatisfaction. 'Therefore, be aſſured that my only anxiety is 'to convince you what eſteem and friendſhip I 'bear towards you.'

FIG. I.

a a The ſhagreen ſkin,
b b The ovular globe,
c c The upper fræna,
d The mouth and teeth,
e e The œſophagus and ſalival ducts at the ſides,
f The large horns,
g The muſcle,
h The genital organ,
i The brain,
l The left lobe,

FIG. II.

a a The ſkin,
b b The ovular globe,
c c The muſcle retractor,
d The globular appendage of the tongue,
e e The long under fræna,
f The ſhort,
g The ſhorteſt,
h The muſcle of the large horn,
i The muſcle of the ſmall horn,
l The mandibular muſcle.

ARTICLE III.—REFLECTIONS.

From the numerous facts in the preceding article, impartial readers will at once perceive that when I first announced, in my Prodromo, the renovation of the head of the snail, I only laid open to the philosophical world an incontestible truth before unknown; and that those authors, who have trusted to controvert my experiments by theirs, have been deceived.

The reproduction which Roos obtained must have been provided with a new brain, a portion of the œsophagus, lips, teeth, jaws, and tongue.

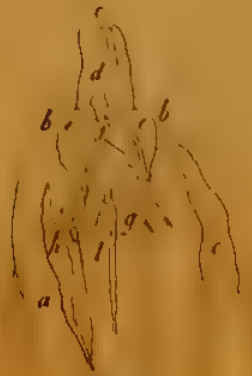
One of M. Lavoisier's snails regenerated a new head exactly resembling the old. The brain must also have been reproduced, because decapitation took place behind the four horns.

No less decisive and judicious were the experiments of M. Turgot, Tenon, and Herissant. It is specifically said, some of the snails were completely deprived of the head.

Muller's experiments wonderfully correspond with those of the Parisian academicians; and when I name Muller, I speak of one of the first German naturalists. The same diligence, sagacity, and circumspection, which, besides the eminent knowledge that characterises his celebrated works,

Fig. 1.

Fig. 2.



works, are alſo diſtinguiſhed in his judicious memoir on ſnails.

Father Scarella, a learned naturaliſt and mathematician, whoſe death Breſcia has ſtill to lament, had the head of a ſnail, decapitated behind the large horns, ſo completely renewed, that, except in colour, it could not be diſtinguiſhed from the old one.

Schaeffer, whoſe name alone is a ſplendid eulogium, does not mention the exact place where the cut was made; therefore it is neceſſary to conſult his diſſertations in German, which I have not ſeen; but he expreſsly declares, that his experiments and obſervations confirmed mine in the moſt ample manner. He alſo obſerves, that he has perhaps extended his curioſity a little further than me, probably alluding to the reparation of the foot. However I had alſo witneſſed the reproduction of that part which ſome call the tail, that is, the poſterior extremity of the foot; and I have even obtained reparation of the whole foot, or the part on which the animal reſts in its progreſſion. This is ſaid in the Prodromo, p. 70, and repeated in the firſt memoir. But I rejoice to ſee the ſame reproduction confirmed by a naturaliſt no leſs expert, ſuch as M. Lavoiſier.

The ſnails deprived of part of the neck, by the Abbé Troilo, periſhed; but thoſe that had loſt the head, and no more, ſurvived, and repaired it

completely. One moſt convincing proof was, that the ſnails began to feed.

The ſame evidence was given by one of the ſnails decollated by M. Senebier, an illuſtrious Genevan, who has publiſhed an excellent treatiſe on *The Art of Obſervation.* His precepts have been enhanced by example; for he has enriched philoſophy with the beſt experimental inquiries, and lately with *Memoirs on the influence of ſolar light on the three kingdoms of Nature.* There could be no doubt of complete decapitation, for he diſſected the ſevered heads.

It cannot be ſuppoſed that the experiments of his countryman were leſs convincing, that is, of Bonnet, whoſe works conſtitute the delight and admiration of the age. How many precautions did he not adopt, to be beyond the ſhafts of his adverſaries! What preciſion did he not uſe to aſcertain that the head was ſevered! What aſſiduity, care, and diligence, in minutely obſerving the phenomena of the reproducing head! With what evidence and impartiality does he not demonſtrate reproduction! But theſe important facts will be highly enjoyed by thoſe who conſult the memoirs of that philoſopher: for I am well aware that any ſketch or extract of mine would be but a faint and languid perhelion.

I ſhould ſtill have to ſpeak of the three letters from Sig. Caldani, Pratolongo, and Girardi, did

I not

I not reflect that the learned will better comprehend the import, on perusing the letters themselves. Sig. Caldani observes, that only four of his decollated snails were alive 27 September 1782. He afterwards wrote to me, 19 April 1783: 'Of the four remaining snails, three died 'during the late cold. The fourth, which was 'the largest of the whole, has for four days shewn 'me a very fine head.'

Sig. Girardi observes, that the moment the horns, particularly the larger, are divided, a stream of cerulean fluid escapes. Both he and I explain this phenomenon, but we disagree in the explanation. In my memoir, I have supposed the fluid first generated in the glandular parts of the horns: he, on the contrary, thinks it is the blood of the animal, or a fluid analogous, that spouts from the divided horn. On better examination of the fact, it appears that he is right and I am wrong. Within the horns, there is truly a large vessel, which even extends over part of the head, full of that light cerulean fluid; and, when the horns are cut asunder, it becomes flaccid and almost disappears, by discharging the fluid whose colour renders it visible.

Passing through Parma last November, I had the pleasure of embracing my most esteemed friend in his own house. He shewed me several snails mutilated during the preceding spring, and

the ſeparation had gone far beyond the head. I was anxious to learn the conſequence, which he was pleaſed to communicate in the following paragraph.

'I decapitated the ſnails that you ſaw on the 12 'of May laſt year, fully four lines behind the 'large horns. In the ſevered head were not only the muſcles, and all the nerves connected to 'the oval globe, the ſalival ducts, brain, &c. but 'behind them the anterior portion of the male 'organs, and more than half the appendages of 'the uterus, including the angular part of the 'ovary, ſo that I thought it impoſſible that theſe 'reptiles could reproduce the mutilations. After 'keeping one, in which the cut had even gone 'further, a long time in my hand, and immerging 'it long in water, within theſe few days it appeared ſtretching out its neck. The rudiments 'of the large horns were evident at the extremity, 'and below them the lips and mouth. Almoſt 'diſcrediting my ſenſes, I was ſtrongly tempted 'to diſſect the ſnail to ſee the internal reproduction; but the external parts being incomplete, 'I reſolved to defer my examination. I have 'ſince kept it ſhut up in the ſame box where you 'ſaw it, that I may diſcover in future what perfection the external parts will attain. Parma, '13 January 1784.'

It

It muſt be obſerved, that a certain degree of heat is required for reproduction, and not leſs than 61°. Reproduction is finally nothing but a new generation, with this ſingle inconſiderable diſtinction, that in ordinary generation an organiſed whole originates and unfolds, while in reproduction only a part of that whole is developed. The ſame conditions requiſite for the origin of the whole are required for the origin of a part, and among theſe conditions is heat. In man, quadrupeds, and moſt other warm blooded animals, the fœtus originates at any ſeaſon, becauſe it is matured in the body of the mother, where there is always a certain degree of heat. Birds may hatch their eggs in winter by means of their heat. It is otherwiſe with cold animals: they do not and cannot propagate except in warm weather; for during the reſt of the year they are in ſuch a ſituation from cold, that, to judge by external appearances, we ſhould rather ſay they were dead than alive. Thus it is with moſt inſects, worms and reptiles, among which laſt, ſnails ought to be numbered. They neither copulate nor generate except in ſummer. Shut up in their calcareous dwellings, and the mouth covered with the operculum formed of that viſcous gluten exuding from the body, they remain motionleſs, and in a lethargic ſtate, under the earth all winter. Not only are they incapable of generating

rating during this ſeaſon, but thoſe that have not attained the full ſize continue without any additional increaſe: therefore they diminiſh both in ſize and weight, as is evident by examining them when they retire in winter and appear in ſpring. The cauſe is manifeſt, as, on the one hand, they do not feed during the whole of this time; and, on the other, their perſpiration is not entirely at an end. Therefore every ſeaſon is not adapted for the organic evolution of the head, but ſummer only and ſpring when advanced.

This Memoir may be terminated with a general reflection. Apparently one chief reaſon why my diſcovery has been ſo much called in queſtion, nay abſolutely denied, is, becauſe that diſcovery reſpects the head. Had it been ſaid that an animal was found with the faculty of reproducing the limbs and tail, and that theſe members were not of very ſimple ſtructure as thoſe of many inſects, but nearly of the ſame conſiſtence as the limbs of quadrupeds, there is no doubt that it might have appeared in ſome degree marvellous. Yet the fact would not have ſeemed ſo far from poſſibility. But was it ſaid that another animal was diſcovered which would reproduce the ſevered head, and if this head had eyes, mouth, jaws, teeth, tongue, nerves, and brain, not only ſhould we be amazed, but the mind aſtoniſhed would revolt and reject the affirmation.

Thus

Thus do we find ourſelves more diſpoſed to judge a fact fabulous than to admit its reality. On tracing the origin of this repugnance of this incredulity, it is eaſy to diſcover that it is ſupported by analogy only. Habituated from infancy to ſee certain animals amidſt which we live, as quadrupeds, birds, reptiles, amphibia and inſects, from particular ideas of theſe, we form general ideas of the whole maſs of animals. Therefore when the head is ſevered from a dog, a pigeon, a ſerpent, frog, or fly, inſtead of being regenerated, we ſee the animals die, and we ſuppoſe the ſame muſt happen to a ſnail and every other animated being. Thus do the multitude judge, and thoſe of the literati who think no deeper than the multitude. But it is not ſo with the real philoſopher and naturaliſt; with him who has ſeen how much has been obſerved by able inveſtigators in the unlimited kingdom of animals, who beholds innumerable anomalies and exceptions, and conſequently learns how fallacious analogical argument muſt be: the recital of this phenomenon will excite his ſurpriſe, as we cannot but wonder at any unexpected novelty; yet he will not deny the poſſibility of the fact, from the numerous other facts apparently no leſs revolting, than that of regenerating ſnails, which are nevertheleſs moſt true. Such are the prodigies of polypi, earth worms, freſh water worms, aphides,

aphides, gall insects, wheel animals, libellulæ, artificial fecundation, and so many more which rather seem possible than true.

How far have those departed from real philosophy, who have called the reproduction in question, before endeavouring to verify it! Prejudices, whether favourable or unfavourable for any system, theory, or hypothesis, are in general fatal to observation. When we interrogate nature, it must be divested of all prejudice and passion, which obscures the fair face of truth; and, with an amiable indifference, we ought to judge equally against others as ourselves. If, on the contrary, we are prepossessed with wishes, distrust, and doubts, we shall behold experiments from the side favouring our desire, and not from that adverse to it. Our opinions will be incorrect; and, instead of adding useful facts to philosophy, we shall increase the number of errors.

EXPERI-

EXPERIMENTS

ON THE

REPRODUCTION OF THE HEAD

OF THE

TERRESTRIAL SNAIL,

BY

CHARLES BONNET.

MEMOIR I.

THE only object of publiſhing my experiments on the reproduction of the head of the ſnail, is to afford an additional confirmation of the Abbé Spallanzani's beautiful diſcovery. How much it has been diſputed out of Italy, and particularly in France, is well known. There are naturaliſts of

of my acquaintance, who, after decapitating hundreds of ſnails without ſucceſs, have conceived themſelves entitled to conclude, that the Italian obſerver had allowed fallacious appearances to impoſe upon him. One, in a letter to myſelf, did not heſitate to reproach me for inſerting an account of the imaginary diſcovery in the *Paliŋgeneſie*, and for reaſoning on it as a fact completely aſcertained. It will juſtly be thought, that theſe reproaches did not in the leaſt impair that confidence with which the ability and ſound reaſoning of the Reggian naturaliſt had inſpired me. Beſides, he had communicated a full account of his intereſting experiments in a courſe of correſpondence, and it was eaſy for me, only by a trial of the facts, which the learned obſerver had ſeen and ſeen again, to judge of the prodigies he laid before me, and which he ſoon preſented to the public in an Italian Tract 1768, which in the ſame year was tranſlated into French. But the author having there neglected to detail the precautions he had adopted, to ſecure his diſcovery againſt all diſpute, I requeſted him to publiſh an account of his method, which he tranſmitted in a letter from Modena, 11 September 1769, and it was printed in the *Avant Coureur* 30 October. This letter, though perfectly calculated to remove all heſitation, has had but a partial effect; ſome doubts ſtill remain, and peo-

ple

ple continue oppoſing to the Pavian experiments, thoſe which they ſuppoſe contradictory or apparently confuting them. I have been induced myſelf to repeat the learned Profeſſor's inveſtigations, owing to this conflict of experiments, which has continued nine years. From the account which I proceed to give, the impartial public will judge of the confidence that it deſerves.

The ſpecies of ſnail on which I operated, is of a middle ſize, and frequent in the fields or in gardens after a rainy day; numbers then abandon their dark retreats, and in a ſhort time one may collect hundreds. The ſhell of ſome is yellow, or yellowiſh; on that of others are circular black or brown faſciæ.

It is by no means an eaſy matter to decapitate a ſnail, for the moment it feels the inſtrument, it ſuddenly retires into the ſhell. Thus it is evident, that we may ſuppoſe a ſnail is decapitated, when only a portion of the integuments is cut off. To avoid deception, I have uſed ſeveral precautions. The ſnail is allowed to extend as much as poſſible, and an additional extenſion is procured by immerſing the animal in water. The inſtrument is frequently preſented before ſtriking the blow; and I never eſteem the operation complete unleſs the head is obtained entire, with the four horns fully diſplayed, and alſo the mouth, which may always be recogniſed by the opening

of

of the lips. The head, as it appears ſome time after ſeparation from the trunk, is repreſented a little magnified, fig. 1. plate 8. The two large horns, gg, are ſomewhat contracted; the ſmall, pp, are entirely ſhrunk within themſelves. The mouth b, is cloſely ſhut, and the lips are very evident there.

A ſharp edged knife ſeems more proper for this operation than a ſcalpel; ſciſſars are ſtill leſs convenient than the latter. I have uniformly obſerved to make the cut perpendicular to the axis of the trunk.

Immediately after decapitation, the ſnail retreats far within its ſhell, and in general does not appear again. A copious portion of that viſcous fluid, with which it is ſo amply provided, is now diffuſed. Some motion is perceptible in the horns of the ſevered head, and chiefly in the larger; but this very ſoon ceaſes; and I have in vain tried to renew it by ſtimulating the head near the origin, with a ſcalpel. All the horns contract to a certain degree inſtantly on the operation, and the ſmall contract more than the others.

By a very ſimple method, one may aſcertain whether the operation is complete, namely, by immerſing the decapitated ſnail in water. It quickly leaves the ſhell, and extends as much as before decollation. Then we immediately ſee whether

whether the trunk is entirely deprived of the head (1). The anterior part of such a trunk, drawn from life, is represented, fig. 2, and the profile, fig. 3. plate 8. It is evident that the flesh is powerfully contracted to close the enormous wound.

The viscous matter, exuding after decapitation, forms a thin whitish operculum, which completely obstructs the mouth of the shell. Two of these opercula are frequently formed, one situated above the other, sometimes there are three; the exterior of which is near the edge of the opening, and the inmost more or less within the shell.

Though the decapitated snail can reproduce several opercula, the viscous fluid is gradually exhausted, and the shell at last remains open, or nearly so, because the animal, being incapable of feeding while deprived of the head, cannot repair the continual loss of this kind of varnish. It insensibly becomes emaciated, which is evident

(1) It may happen that the snail does not extend so much as would be desirable, or as is necessary for judging of the progress of reproduction, but it is only requisite to take the shell between the fingers, after removing it from the water, and the animal will soon extend as much as possible. Great care must be observed to avoid touching the snail, because the most gentle motion makes it retire into its shell.

by the diminution of ſize and an internal tranſparency. I have often been aſtoniſhed at the number of ſucceſſive opercula produced by decapitated ſnails; however, all do not produce them; at the ſame time, the ſhells of very few continue open.

My decapitated ſnails were kept in boxes. Some remained at the bottom, others attained the ſides, againſt which they applied the mouth of the ſhell; others, aſcending higher, reached the covering, where they fixed in the ſame manner. Theſe ſeemed to be the moſt vigorous, or had ſuffered leaſt from the operation.

When I wiſhed to learn the ſtate of the decapitated ſnails from week to week, I had only to take the opercula carefully from the mouth of the ſhells, and then immerſe them in very limpid water; thus the ſnails are forced to appear ſooner or later, but they have ſometimes remained within for ſeveral hours after immerſion. This method, to which I have always had recourſe, is the beſt in my opinion, for the ſnails, then extending to the utmoſt limits, expoſe the anterior part ſo completely, that nothing can eſcape the obſerver's eye. They endeavour to leave the water, and gradually attain their purpoſe, if the depth is not too great; or, crawling ſlowly over the bottom and ſides of the veſſel, they advance until they reach a dry poſition, and

fix

fix themſelves there; and it is neceſſary, for forcing them out, to re-immerge them in the water. Notwithſtanding privation of the head, they advance forward as if they had one, only their progreſs is a little ſlower.

At firſt, I decapitated only a dozen of ſnails, which was on the 8 of May 1777. I repeat, and it cannot be too often repeated, for I am earneſt to obviate the moſt trivial objections, that decapitation has never been eſteemed complete but when I had the head entire, and accompanied by all its appendages, on my tablet. The heads ſevered in this manner were ranged together along one ſide of my tablet, where they ſtill remain.

Let us proceed, in the next place, to deſcribe the wonderful reproductions which ſucceeded before my eyes. I will not enter into minute detail; it is unneceſſary, for my only purpoſe is to prove the reality of the reproduction, in confutation of the detractors of the famous diſcovery of my friend the Abbé Spallanzani.

This reproduction does not preſerve the ſame uniformity as that of the head of thoſe aquatic worms, which I multiplied by ſections in the year 1741, and of which I publiſhed an account a few years afterwards. The reproductions of the ſnail preſent a number of varieties, which it would be tedious to deſcribe. Signor Spallan-

zani has given ſome inſtances in his *Prodromo*, and after him, I have mentioned them in *la Palingeneſie*, part 9; the reader is referred to theſe two works, for I ſhould here confine myſelf to my own obſervations.

A profile of the anterior part of a ſnail decapitated 8 May, and delineated 21 June, is a little magnified, fig. 4. The two large horns, *g*, *g*, are beginning to protrude; the left is further advanced than the right, which is juſt originating. A brown, or blackiſh line proceeds from the large right horn; this is the optic nerve and its muſcle; the various motions and ſtructure of which, Swammerdam has diſplayed to our admiration. Some tranſparency is perceptible in the fleſh; and, as it increaſes greatly in ſnails that have abſtained a month or two from food, the optic nerve and muſcle become much more evident. A white line *l*, runs along the back; but I am ignorant whether it is a veſſel. The anterior part of the ſame ſnail, viewed from before, is repreſented from the life, fig. 5: only the upper extremity of the large horns is ſeen, and at the extremity there appears a minute black point; this is the eye of the ſnail, in which Swammerdam aſſures us he has found the three humours of an eye, the two tunics, the uvea, and arachnoid. Here the eye is already viſible; though the horn is but in its origin, it is perceptible in

thoſe

thoſe that have even made leſs progreſs, as will immediately be obſerved. The ſmall horns have no eye at the extremity; they do not yet appear, neither are the new lips of the mouth *b* viſible. This ſnail I ſhall diſtinguiſh by the letter A.

The anterior part of another ſnail, drawn 23 June, is repreſented a little magnified, fig. 6. Reproduction is ſomewhat advanced, for one of the ſmall horns, *p*, ſeems completely regenerated, though its fellow has not begun to protrude. The origin of the large horns, which have made very little progreſs, is ſeen above the ſmall horn *g*, *g*. This is a ſtriking example of the varieties which occur in the reproduction of the head of the ſnail; one of the ſmall horns is far advanced, while the correſponding horn is ſtill imperceptible, and the larger ones only beginning to expand. Fig. 7 is a profile of the ſnail. The tranſparency allows the optic nerve *t* to be ſeen proceeding from the origin of a large horn; the eye is diſtinct, and the lips of the new mouth *b* are viſible. This ſnail I ſhall diſtinguiſh by the letter B. The anterior part, as on the 2 of July, is repreſented a little magnified, fig. 8, and the ſection, fig. 9. The mouth *b* cannot be miſtaken, nor the large horns with their eyes, *g*, *g*.

Another ſnail, which on the 23 of June ſeemed to have completely repaired the head, ſhall be

deſigned by C. The four horns were perfect, and had acquired the natural ſize of thoſe proper to this ſpecies. The mouth ſeemed to be regenerated: the opening was complete; and the new lips, very diſtinct, were of the figure and proportions which they ought to be. In a word, this ſnail ſo much reſembled other ſnails of the ſame ſpecies, which had not been mutilated, that I could diſtinguiſh it only by the tranſparence and diminution of ſize. It is repreſented fig. 10. as drawn from the life: and the plane of the anterior part, fig. 11. where the new mouth and its lips are diſtinctly ſeen. Above it, the tranſparence of the fleſh ſhews an oblong ſpot *t*, which is the teeth of the ſnail: the lips can approach to it or recede. Theſe two figures were not deſigned till towards the middle of ſummer. From the 23 of June, I began to ſupply the ſnail with young vine and lettuce leaves: but it would not touch them. After traverſing the leaves and the ſides of the veſſels, it commonly fixed to the covering, and remained there during complete weeks. Notwithſtanding its abſtinence more than two months in ſummer, it always ſeemed in good health, and is ſtill in the ſame ſtate while I write this, 21 July.

I have already obſerved, that the eyes appear, although the large horns are but beginning to be repaired. This was evident in a ſnail decapitated

ed 8 May, whofe head is repreſented, fig. 1. On the 6 of July, when it was deſigned, the reproduction had made very little progreſs, fig. 12. Though the origin of the large horns is viſible, they do not yet begin to extend: indeed, the eye alone, which is already perceptible, indicates their place: it appears like a minute black point, ſuch as it is poſſible to make with the fineſt pen. The ſnail was drawn when ſtretched to the utmoſt, and I have uſed the ſame precaution with all the reſt. In that of which I now ſpeak, neither the ſmall horns nor mouth yet appear.

When the large horns are retracted, the black point or eye is eaſily perceived within through the fleſh. I have frequently diſcerned it with my naked eye, and even in thoſe ſnails whoſe reproduction was very little advanced.

I ſhould not neglect to obſerve, that of the twelve ſnails decapitated 8 May, only one died. All the reſt ſtill ſeem to be well, 27 July: but the progreſs of reproduction is very various. In ſome, it is no more than begun: in others, the large horns alone are repaired, the origin of the ſmall ones is imperceptible, and the mouth is ill defined. Some of the large horns are only a half or two-thirds of a line in length. Such are thoſe of the ſnail, which has hitherto been diſtinguiſhed by the letter A: the anterior part, as it appeared 21

June, is represented fig. 4. and as it was 26 July, fig. 13. Something singular is presented by the large horns: they are thicker in proportion to their length than those of unmutilated snails. At the extremity, we remark a kind of deformity, which seems produced by a certain plication of the flesh, and gives the horns a monstrous appearance. However, the eye is very distinct. The colour of the horns tends to violet, which is generally the case with all reproducing horns, and very much resembles that of the nerve traversing the flesh. The mouth is not perceptible in the anterior part of another snail, unless as a prominence arising, *b*, fig. 14.

I have commonly remarked, as well as Sig. Spallanzani, several conspicuous irregularities in the reproduction of the double parts of the same snail. Sometimes a large horn only half or two thirds of the length of the corresponding horn appears, or the latter is scarcely visible. Likewise a small horn will be completely regenerated, while its fellow is hardly perceptible or not at all. I have seen one half of a lip reproduced, and the other fully repaired.

I may limit myself to these few examples: they will be sufficient to give an idea of the varieties presented by the regenerating head of the snail. It seems to me, that they infer the reproduction of one part to be independent of that of another:

another: for how can we deny it, if one horn is completely reproduced, while the reſt are inviſible, or only beginning to grow? This muſt be conſidered a moſt important fact in the theory of theſe admirable reproductions: but I ſhall omit ſpeaking of it here, as I have already endeavoured to ſketch it, Part 10 of the *Palingeneſie*.

On the 12 of May, I decapitated thirty ſnails of the ſame ſpecies, treating them preciſely ſimilar to the firſt: above two-thirds have periſhed. Thoſe ſtill alive regenerate variouſly, and preſenting the ſame varieties, or ſome analogous to thoſe already deſcribed. The months of May and June, and the beginning of July, have been very raw and wet. At ſun-riſe, Reaumur's thermometer ſtood at 4, 5, 6° above freezing on ſome days in the firſt week of July (1).

At preſent, this diſcuſſion ſhall be no further extended, for I intend to reſume the ſubject in another Memoir: but enough has been ſaid to prove that nothing is more certain than the wonderful reproduction. I know not what to think of the fruitleſs attempts of ſome philoſophers, and particularly thoſe of Meſſrs Adanſon, Cotte, and Bomare. Perhaps they have too ſoon declared the ſtate of their experiments, or taken for

(1) About 41°, 43°, and 45° of Fahrenheit's thermometer.—T.

for equivocal what was a real reproduction: or perhaps they thought the ſnails ſtill alive were dead. In this caſe, it is requiſite to have much patience, and, above all, to deſpair of nothing. I do not ſpeak of the diverſities which the difference of ſpecies might occaſion in the reſult of the experiments made by theſe celebrated perſons; for there is reaſon to ſuppoſe, that among the immenſe number of ſnails on which they have operated, ſome were of the ſame ſpecies as mine. Neither do I ſpeak of the diverſities that might ariſe from the difference of climate; for that of Paris is very little different from ours. Therefore I entreat theſe able naturaliſts not to be diſcouraged, and to reſume a ſubject ſo pregnant with new facts, and one which cannot be too deeply inveſtigated. They poſſeſs far more information, talents, and ability, than are neceſſary to ſucceed in experiments of this nature: and I may predict the moſt complete ſucceſs if they will not be diſcouraged, and if they will proceed in the manner I have done.

M. Adanſon wrote to me concerning his own experiments, 30 July 1769. 'I begin to have a 'philoſophic doubt concerning the regeneration 'of the head, horns, and jaws of ſnails. My ex- 'periments, diverſified to infinity for above a 'year, on fourteen or fifteen hundred ſnails of 'different ſpecies, convince me that my doubt has 'foundation.

'foundation. I have, as every one has had, re-
'productions, even very immediate ones, of
'horns, heads, lips, and other parts; but theſe
'were reproductions of parts that had not been
'entirely cut off: for all the heads, I ſay, the
'real heads, all the horns, all the jaws, and the
'other parts which have been completely cut
'away, and only a quarter of a line from the
'origin, never exhibited any kind of reproduc-
'tion, far leſs a complete regeneration. Let us
'be ſtrict, and inveſtigate the truth. All who
'have mutilated ſnails, and firſt Sig. Spallanzani,
'have certainly been deceived. They have thought
'the head was ſevered, when the cap only has
'been cut off: they have believed that they ſe-
'parated or eradicated the horns and jaws, while
'the origin always remained; whence it is not
'wonderful if reproductions enſued. Theſe, you
'will candidly admit, are not reproductions, or
'rather regenerations, ſuch as you, M. Trembley,
'and Reaumur, had ſeen in freſh water worms,
'the polypus, the claws of lobſters.—How many
'well credited operations have deceived perſons,
'leſs familiar than us with ſimilar operations and
'the anatomy of ſhelled animals! They have
'thought that they had completely cut off ſo many
'heads, horns, and mouths, beyond the origin,
'which, in every journal and periodical paper,
'they have ſo liberally regenerated. I am well

'aware

'aware of our deficiency in moſt nice experi-
'ments; and, notwithſtanding my great expe-
'rience, I may almoſt preſume to ſay, dexterity
'in the anatomy of the ſmalleſt animals, I always
'diſtruſt myſelf. For this reaſon I have repeated
'the ſame experiments an hundred and an hun-
'dred times, before hazarding the reſults before
'the public. I have laboured the firſt, or among
'the firſt, to corroborate all the experiments of
'Sig. Spallanzani, and to make additions to
'what might have eſcaped his notice. I have
'operated on a greater number of animals, and
'diverſified my experiments more than any other
'perſon, to judge of all that has been read be-
'fore the academy, or printed; and I am the
'only one who has read nothing on the ſubject,
'which I inveſtigate with the utmoſt aſſiduity.—
'It is nearly the ſame with the reparation of the
'parts of newts, ſeveral ſpecies of frogs, toads,
'tadpoles, &c. When part of the tails and feet
'were amputated, I have ſeen ſenſible reproduc-
'tions; but none when theſe parts were am-
'putated cloſe to the origin. Conſider my ex-
'preſſions well, *regeneration* and *origin*, on which
'your principles reſt ſo much, and there is no
'actual reproduction. And I hope you will do
'that juſtice to my doubts, as to acknowledge
'that Sig. Spallanzani and his followers have
'too far extended their expreſſions of regenera-
'tions,

'tions, which were only partial reproductions of 'parts.'

To the numerous experiments and doubts of my eminent correſpondent, I ſhall oppoſe only the letter to me from the Abbé Spallanzani, cited in the beginning of this treatiſe, wherein he details all his precautions to avoid error. I ſent M. Adanſon a copy of this letter, but it did not produce that effect on his mind which I expected; nay, he ſtill perſiſted in his doubts when he wrote to me, 20 July 1775. 'The various 'parts amputated, or torn, not only from differ-'ent ſpecies of ſnails, but alſo from ſeveral other 'aquatic animals, as frogs, toads, newts, have 'produced no organized reproduction to me, as 'the mutilated part did to Sig. Spallanzani. I 'have diverſified the experiments, which my 'friend Mr Needham and ſome other obſervers 'of this rank have witneſſed, to ſuch a degree, 'that we all eſteem it certain, when the operation 'has been complete, the reproduction is but a 'ſtump, that is, a maſs of fleſh unorganized or 'differently organized. And Signor Spallanzani 'ſhould know that the obſervations of our moſt 'celebrated anatomiſts have proved, that the re-'production of the tails of lizards, which are ſo 'common, although externally well formed, 'preſent no regular oſſification as the reſt; nor 'have they any internal vertebræ.'

M. Adanſon

M. Adanſon is evidently one of thoſe philoſophers who ſtart difficulties about facts; and who deſire themſelves to behold prodigies again and again, before admitting them for truth. This reſerve cannot be cenſured; but I confeſs that here it appears extreme, eſpecially after evidence ſo ſtrictly demonſtrative as the Abbé Spallanzani has given of his diſcovery. May I therefore hope, that the experiments, which I now publiſh, will triumph over the incredulity of our learned academician? Doubtleſs he will not ſuſpect that I have deprived the ſnails only of the *cap*, to uſe his own expreſſion; for the head ſo completely and ſo perfectly ſeparated from the trunk, as repreſented fig. 1, will not allow the leaſt ſuſpicion to remain. I muſt requeſt M. Adanſon to conſider all the details of my experiments; and to attend to the able deſigns of the artiſt, which admirably repreſent the regenerations I have witneſſed. They might eaſily have been further extended, but I do not eſteem it neceſſary to the purpoſe I have in view. Should it be objected, that the ſnail drawn from the life, fig. 10, 11, had not then touched the young vine and lettuce leaves with which it was ſupplied; I may anſwer, that it has given the moſt indiſputable evidence of being provided with very good teeth; for, on the 27 of July, it began to eat the paper covering the mouth of the veſſel where it was confined,

confined, and voided ſome well formed excrements, the colour and conſiſtence of which being exactly like paper, indicated that they were its remains.

M. Adanſon alſo doubted the reproduction of the members of the newt, which has been ſo fully aſcertained by the numerous experiments of Signor Spallanzani, and the principal reſults publiſhed in his intereſting Proſpectus 1768. In the letter of 20 July 1775, M. Adanſon obſerves, 'Whenever the operation on newts has been 'complete, only the ſtump of a reproduction ap-'peared, that is, a maſs of fleſh unorganized or 'differently organized:' and he cites the teſtimony of Mr Needham, and ſome other obſervers. But what will M. Adanſon himſelf ſay, when I inform him that this pretended ſtump, or imaginary lump of inorganic fleſh, is the member itſelf perfectly formed, concealed under this deceitful appearance; and which has been completely developed before me, as I had formerly ſeen the evolution of the head and tail of thoſe aquatic worms, which were multiplied by being cut in pieces. In my cabinet, there are actually newts completely repaired, of which I ſhall publiſh a hiſtory in a future memoir, accompanied by excellent deſigns. Our celebrated academician has therefore been precipitate in his opinion, when he thought he only ſuſpended it. He has

decided

decided that the newt reproduced but a ſtump; while the ſtump was the member itſelf, where nothing eſſential was defective, and it had juſt to acquire the ſize of that which it replaced.

Thus M. Adanſon was as much deceived concerning newts as concerning ſnails; and the miſtakes of ſuch a naturaliſt are a good leſſon to thoſe who have neither his knowledge nor his ability. I am convinced that he will acknowledge his error, for I know him to be a ſincere friend to truth; and ſtand in no fear of reproach for having laid it open in this little treatiſe.

M. de Bomare, no leſs a friend to truth, and whoſe experiments have been as unſucceſsful as thoſe of M. Adanſon, was conſequently equally incredulous. I had referred him to the ſame letter from the Italian obſerver, printed in the *Avant Coureur*, 30 October 1769; and, on the 5 of November 1775, he replied—'You aſk me why 'I have not anſwered one of the articles of a for- 'mer letter reſpecting the reproduction of the 'head of the ſnail. I can anſwer you, that all 'the experiments which I have attempted on this 'ſubject ſeem adverſe to thoſe of Sig. Spallanza- 'ni. You will ſee, at the article Limaçon of my 'Dictionary, what I have ſaid on the ſubject, and 'which I mentioned before in 1768.' I ſhall tranſcribe

ſcribe from the *Dictionnaire d'Hiſtoire Naturelle* the paſſage to which M. de Bomare refers.

'I confeſs that, being unable to credit the re-
'production, I made many experiments on the
'ſubject while at the Chateau de Chantilly dur-
'ing Autumn 1768, which have ſince been com-
'municated to the public; and the reſult fol-
'lows. I decapitated fifty-two terreſtrial ſnails:
'all, whenever they felt the ſharp-edge of the
'knife, ſuddenly and very powerfully contracted
'themſelves, and the operation finiſhed, the part
'that retired precipitately into the ſhell appeared
'corrugated like the extremity of the rectum of
'a hen. Nine were twenty-four hours in mo-
'tion, and only thoſe which had an imperfect cut
'on the neck between the large horns and the
'organs of generation; the knife being ſo blunt
'that I had evidently ſeen all the horns retract
'into the interior of the animal. Thus I had
'only ſeparated the ſkin and jaw of the ſnails;
'ſo that in ten or twelve days, they proceeded
'from the ſhells crawling about with mutilated
'horns. The ſnails, which loſt the diagonal half
'of the head, diſplayed no more than two horns:
'but all thoſe that I had completely decapitated,
'which were by far the moſt numerous, died in
'a few days, except two, which lived five months
'fixed to a wall, and died in ſpring without in-
'dicating reproduction of the head. I have

'made a longitudinal incision between the four 'horns in the head of other snails: nature em- 'ployed above a month in uniting the parts, and 'the animals were very languid. These experi- 'ments were repeated, 1769, and all unsuccessful- 'ly. Many persons have informed me, from dif- 'ferent countries, that their essays have been ex- 'actly similar to mine.'

It is singular that I have succeeded with only a dozen of snails, while M. de Bomare has failed with more than fifty, and M. Adanson with above fourteen hundred. But it is possible that these gentlemen were too earnest to believe that their experiments had failed; or they did not pay sufficient attention to the progress of the regeneration, always more or less tardy, more or less disguised, and, consequently, more or less difficult to be recognised.

I have named another valuable naturalist, F. Cotte, curate of Montmorency, who has been equally unfortunate as M. Adanson and Bomare. His unsuccessful experiments are related in a letter to the Abbé Rozier, which was published in the *Journal de Physique*, May 1774. The reproductions of heads are according to him *imaginary*. Because he decapitated a great number of snails from 1768 until 1774, and almost the whole died soon after the operation, which had been performed with a sharp knife, not by drawing but by

by a ſingle blow; and his account is concluded with remarking three conſequences, which he aſſures us were attendant on all his experiments and obſervations. 1. He remarks that ſnails have the property of contracting very ſuddenly to protect their heads from the inſtrument, ſo as to eſcape with loſing only part of the horns, or at moſt the ſkin of the head. 2. When it happens that the head is actually cut off, it is not reproduced: at leaſt he declares that he has never ſeen reproductions, not even of portions of the horns, amputated. 3. Snails can live very long without eating and without the head.

I am ignorant whether M. Adanſon, de Bomare, and Cotte have continued their experiments, or what has enſued. But I am not the only naturaliſt who has ſucceeded in corroborating Signor Spallanzani's diſcovery: that has already been done by the celebrated Signora Baſſi of Bologna, by M. Lavoiſier and Schæffer; and M. Senebier paſtor and librarian of our Republic, who has given public teſtimony of his ſkill in philoſophy and natural hiſtory, has had the ſame ſucceſs as myſelf in experiments on ſnails. I would here tranſcribe what he has communicated concerning them, did he not ſay that his obſervations are ſent to be publiſhed in the Abbé Rozier's *Journal*.

Although the head of the ſnail is a very complicated little machine from its ſtructure, as I have ſhown in another place (1): it is indubitable that the gelatinous quality of the fleſh greatly promotes its wonderful reproduction. When treating of the polypus, I have enlarged on this obſervation. However, I do not mean that one ſhould conclude that all gelatinous animals, all animals in their primitive ſtate of jelly, may reproduce or repair the loſs of their members, as the polypus and ſnail. Experiment alone can diſcover the limits of this admirable property; and what we have already learned concerning the extent of its dominion ſhould excite naturaliſts to diverſify to the utmoſt their eſſays on a ſubject ſo fertile in wonders. I cannot exhort them too earneſtly to deſpair of nothing even in the moſt uncommon experiments.

(1) *Palingeneſie Philoſophique*, Part 9. new edition of *La Contemplation de la Nature*, Part 3. chap. 21. Note 4. 5.

MEMOIR

MEMOIR II.

HAVING myſelf beheld ſome of the prodigies preſented by the regenerating head of the ſnail, I wiſhed to know what M. Adanſon thought of the reſult of my experiments, and I requeſted him to read the preceding treatiſe, which was publiſhed in the *Journal de Phyſique* 1777. He did ſo, and wrote me on the 10 of January 1775.

'I have read your excellent Memoir on the 'reproduction of the head of ſnails, with all the 'attention it deſerves; and the peruſal only tends 'to confirm me more and more, in what a courſe 'of experiments, continued nearly ten years, 'ſince 1768 until the preſent day, on ſeveral 'thouſand ſnails, has taught me, namely, that 'when the integral parts of ſnails, whether the 'head entire, the eye or the oculated horn, the 'upper jaw are radically extirpated, or cut be-'low the origin, they are not reproduced, either 'under the ſame form, or with the ſame organi-'zation as before. You have decollated two or 'three dozen of ſnails, 8 and 12 May 1777. I 'believe it. You ſevered the heads, as I myſelf 'did, two lines beyond the origin; that is, to-'wards the opening of the parts of generation.

'You have taken from each ſevered head, as I 'have done, the upper jaw entire, and the ocu- 'lated horn alſo entire; then after two or three 'months, in June and July, you have ſeen a 'third of the ſnails, even eleven of the twelve 'firſt decapitated, reproduce a complete head, 'with the eye horns, and the upper jaw formed 'like a horſe-ſhoe, with the ſerrated teeth. You 'muſt permit me ſtill to retain my philoſophic 'doubt concerning the three laſt aſſertions, 'until repetition of the following experiments 'on which it is founded; experiments which 'have ſerved to confirm the accuracy of my 'operations, and certain proofs that my ſnails 'were completely, and not apparently, decapitat- 'ed. To obtain the ſame certainty, take the 'greateſt number of ſnails you pleaſe (that is, 'hundreds, to provide for the great mortality 'that will enſue) not of the ſmall ſpecies, called 'the lacquey, which you made uſe of, and is 'moſt deceiving, from its great lubricity and 'agility in evading the knife, an agility propor- 'tioned to its ſmallneſs, and muſt have miſled 'you; at leaſt, that happened to me in my firſt 'attempts, and obliged me to abandon it; take, 'I ſay, the large yellowiſh ſnail of the vine, nam- 'ed pomatia, or rather the brown garden ſnail, 'by us called the gardener, which is almoſt as 'large, and the moſt common of all. After keep-

'ing

'ing the whole a day or two, more or leſs im-
'merſed in water, and under a preſs, in order to
'diminiſh their vivacity and lubricity, tear away
'the upper jaw, which is formed like a horſe-
'ſhoe, and bounded by five or ſix teeth; the
'lower palate, which is a membrane dentated
'like the tongue of a cat or a file, and eradicate
'the two large oculated horns, uſing for theſe
'laſt little pincers, with thread or flax, to take
'off any edge, and prevent them from ſlip-
'ping, or, indeed, preſſing the neck of the ani-
'mal with two fingers, to eradicate the jaws;
'avail yourſelf of this forced poſition to ampu-
'tate with a fine and ſharp botanical ſcalpel, the
'two oculated horns, with the bulb below the
'eyes; or you may ſeparate only one of the
'horns, on purpoſe to leave an object for com-
'pariſon. Cut the head entirely off others, ob-
'ſerving whether the ſevered heads have the
'jaws and eyes complete. Reſerve the whole
'jaws and eyes, that you may be certain you
'have as many as ſnails operated upon. The
'ſnails, thus deprived of teeth, eyes, or heads,
'will generally live ſix months, even one or two
'years, without food; they gradually become
'emaciated even unto perfect extinction; if, dur-
'ing this time, they recover new eyes, new jaws,
'a new head, which I have never had the good
'fortune to ſee in thoſe identified after the ope-

'ration; if this experiment, made with all the 'precautions I have taken, and which I believe 'it essential to use, succeeds in your hands, and 'with the Abbé Spallanzani, I shall esteem it a 'fact, that the parts entirely separated are repro-'duced in these animals. But, beware, would you 'desire to make such contradictory experiments, 'though without them you cannot be certain of 'the real reproduction of a jaw, an eye, or a 'head. I abridge the subject, because the con-'sequences that may be deduced are in my let-'ter 30 July 1769, which I thank you for hav-'ing reminded me of in your treatises.

'Let us proceed to newts. I have yet been 'unable to peruse your Memoir; but, excepting 'the tail, to judge by my observations on that 'of lizards, it seems incapable of reproduc-'ing the osseous vertebræ; and although I 'have only had reproductions of stumps of the 'feet cut off several species of these animals and 'of frogs, from not being able to prosecute 'my experiments with such conveniency, or 'so long on them as on snails, I most firm-'ly believe the possibility of the reproduction of 'the toes and their bones, when the anterior or 'posterior part of the arm is not amputated.'

I want words to express all the astonishment which this letter from my learned correspondent excited; and I doubt not that the reader will be equally

equally ſurpriſed. What M. Adanſon deſires me to do is preciſely that which would occaſion the failure of the experiment; for, how could one tear out, or eradicate with pincers, the different parts of a ſnail, without cauſing the greateſt internal diſorder? How is it poſſible, in this way, to ſucceed in eradicating all the parts? and, ſuppoſing that it was practicable, ſhould we not endanger the ſources of reparation? Is it not enough, that I am certain, by the moſt attentive examination of the heads I have cut from my ſnails, that they contain all the parts which characteriſe a head, ſuch as the four horns, the mouth, the jaws, &c.? Was it neceſſary to cut out the large horns with a botanic ſcalpel, in order to aſcertain that the ſnail would produce new ones? Was it not ſufficient, that I had once and again beheld the origin and progreſs of the new oculated horns, that I had ſeen the new eye and the optic nerve firſt appear in this wonderful reproduction? It is improper to magnify, as M. Adanſon has done, the alacrity with which the ſnail retracts its head the moment it is touched by the inſtrument, for that alacrity is not ſo great as to prevent a perſon, with a little addreſs, from effecting complete decapitation. I can even affirm with truth, that it very ſeldom failed, when the precautions mentioned in the former Memoir were taken.

M.

M. Adanſon ſeems to reproach me with uſing ſnails of too ſmall a ſize; he remarks, that the ſmall ſpecies which were uſed muſt have deceived me, from their great alacrity in eluding the edge of the knife. Neverthelefs, I can affirm, that I have operated on the ſmall ſpecies as eaſily as on the middle ſized, and even the largeſt. But we exaggerate the alertneſs of ſnails, in ſaving their heads; for, abſtinence during ſeveral days, and immerſion in water, undoubtedly weaken them, and, to a certain degree, diminiſh the celerity of their motions. Beſides, if ſmall ſnails were mutilated, it was only becauſe I reaſonably preſumed, that the wonderful reproduction which I wiſhed to behold would be accompliſhed more eaſily, or in ſhorter time, than in the largeſt ſnails. However, experiments on the largeſt ſpecies have not been omitted; and the ſucceſs ſhall be related.

To terminate this anſwer to the objections of our celebrated Pyrrhoniſt, I ſhall here ſubjoin an extract from a letter that I wrote to him 21 January 1778.

'If it was in the compaſs of my power, my 'dear and illuſtrious friend, I ſhould make the 'experiments you deſire on ſnails. But, in truth, 'I do not think that any thing can be done more 'ſtrictly demonſtrative than what has been ſo 'well executed by my friend Signor Spallanzani; 'and

'and what he narrated at length, in the letter of '11 September 1769, that I tranſmitted to you, 'but to which you have never replied a ſingle 'word.

'What is required? To aſcertain whether the 'head is completely cut off; and whether the 'head reproduced contains all the organs of the 'original head. What have we to do, that theſe 'two facts may be proved? The ſevered head 'muſt be carefully diſſected, and the interior ex-'amined with the utmoſt attention, that we may 'be ſatisfied it contains all the organs belonging 'to it; and it is neceſſary to diſſect the repro-'duced head with equal care, and, by an accu-'rate examination of the interior, to aſcertain 'whether it actually contains every part pertain-'ing to the head of a ſnail. This has frequently 'been done by Signor Spallanzani; and now 'let me aſk, whether, in ſtrict logic, there is any 'foundation for doubting an experiment made 'with ſimilar precaution? Yet you tell me, 30 'July 1769, that the Abbé Spallanzani muſt be 'deceived; that he ſuppoſed the whole head was 'ſevered, when only the cap was cut off. You 'perſiſt in the ſame aſſertion, 30 July 1775. 'Certainly you cannot have attended to the 'Reggian obſerver's letter, to which I referred 'you; therefore allow me to refer you to it 'again.

'I have

'I have set apart all the severed heads, and 'considered the whole attentively. I have re-'marked the two large horns with their eyes, 'the small horns, the mouth, the lips, and the 'other parts: I have then seen new horns pro-'trude: I have seen the eyes of the horns, and 'the optic nerve of the eyes: I have seen a new 'mouth, new lips, and new teeth appear in the 'snails—in the same snails whose original heads 'I had set apart. I have seen snails that gnawed 'the covering of a vessel with their new teeth, 'and void excrements containing the matter 'which they had consumed. What more could 'you desire, my worthy friend? And, after so 'many proofs, how can you inform me that you 'still retain your philosophic doubt? Can such 'a doubt, extended so far, and calling in ques-'tion the most accurate, reiterated, and demon-'strative experiments, be termed *truly philosophi-'cal?* Consider that the Abbé Spallanzani 'and I are not the only observers who have 'beheld the prodigies visible in the reproduc-'tion of the head of the snail. The celebrat-'ed Signora Bassi, Messrs Lavoisier, Schaef-'fer, Muller, and others, have also seen and 'described them. Are you willing to think, all 'these observers have been imposed upon;—these 'who have given such ample evidence of their 'ability and accuracy?

'With

'With reſpect to the reproductions of newts, 'you tell me that you firmly credit the renewal 'of the fingers and their bones, ſo long as the 'anterior or poſterior part of the arm is not cut off. 'I regret that you have written this, before per-'uſing my treatiſe on the *Reproductions of Newts*. 'There you would have ſeen that I amputated 'the fingers, the hands, the cubit, whole arms, 'legs, feet, and thighs; and that all theſe mem-'bers were perfectly regenerated by the animals. 'This would have induced you to put more con-'fidence in the beautiful diſcoveries of the Abbé 'Spallanzani on ſnails and newts. My treatiſe 'was printed in Roſier's *Journal*, laſt Novem-'ber; and I wonder that you, who reſide in the 'ſame place where it is publiſhed, have not pro-'cured it. The figures, added to the memoir, 'are very accurate; but the deſigns were ſupe-'rior to the engravings. It will preſent you 'with facts, which, I hope, in a little time, you 'will not oppoſe. See, therefore, and believe.'

The letter from Signor Spallanzani on the mode of operation, to which M. Adanſon had paid leſs attention than it deſerved, is ſo well adapted to convince naturaliſts of the reality of the ingenious diſcovery which it deſcribes, that I cannot avoid tranſcribing it here, as the beſt refutation that may be oppoſed to the detractors of this diſcovery, and as a model of the method

that

that ſhould be purſued in reſearches of a ſimilar nature.

Modena, 11 *September* 1769.

'I thank you, Monſieur, for your informa-
'tion concerning ſnails. On conſidering the
'different reſults of naturaliſts, and particu-
'larly of French naturaliſts, I am of opinion,
'that, beſides their inexpertneſs in the art of
'experiment, the diverſity of the ſpecies of
'ſnails, on which they have endeavoured to re-
'peat my experiments, has, in an eminent de-
'gree, occaſioned a difference in the reſult of
'their experiments. It is certain that all the
'ſnails of Modena reproduce more or leſs; but
'I do not warrant the reproduction of foreign
'ſnails: perhaps ſome among them do not poſ-
'ſeſs this reſource. You will ſee more on the
'ſubject, in my preface to the Italian tranſlation
'of your *Contemplation de la Nature*, which
'will appear this year. It is very probable
'that the ſnails, which have exerciſed the in-
'duſtry of the learned anonymous Frenchman,
'of whom you ſpeak, are in the number of thoſe
'where the reproductive property does not reſide
'in the higheſt degree. I may ſay the ſame of
'the ſnails on which M. de Bomare and Father
'Cotte have operated. But does it thence fol-
'low that I am deceived? To affirm it would
'be a raſh aſſertion, to call it no more. If any

'one

'one attempts to confute me, I will try to de-
'fend myſelf; and my very circumſtantial de-
'tails, as well as thoſe of my friends, will prove
'that I am in no error.

'You obligingly inquire, whether the ſevered
'head truly contained all the organs pertaining
'to the head of a ſnail? To anſwer this impor-
'tant queſtion, I ſhall mention the mode of per-
'forming the experiment. When I diſcovered
'that ſnails enjoyed the prerogative of reproduc-
'ing their parts, I began to diſſect them, pur-
'poſely to acquire a perfect knowledge of their
'anatomy. I wiſhed to make myſelf maſter of
'all the organs compoſing the head. My model
'has uniformly been M. Lyonet's anatomical
'work; and I was provided with the whole of
'his apparatus. The ſnail, which I propoſed to
'diſſect, was killed in water: it then proceeds
'from the ſhell; the four horns are diſplayed,
'and it dies in this poſition, which is the moſt
'favourable for diſſection. It is by this trivial
'experiment that I have been able to convince
'myſelf, that the ſevered head actually contained
'all the parts Swammerdam has deſcribed in his
'treatiſe on ſnails.

'It was only after having ſtudied the ſtructure
'of the head attentively, that I began to mutilate
'the animal; and I proceeded in the following
'manner: The ſnail was allowed to extend fully

'from

'from the ſhell, and diſplay the horns; then the 'operation ſucceeded wonderfully well; and the 'ſevered head will often keep the horns nearly 'as much extended as before decollation, only 'they ſink down and appear feeble; the head 'itſelf contracts and concentrates very much. 'It dilates, if ſoaked ſeveral hours in water, and 'becomes ſoft; in which ſtate it can eaſily be 'diſſected.

'I began the diſſection cloſe to the cut; and, 'after removing the integuments, I could diſ-'tinctly ſee the diviſion or diſtribution of the 'nerves proceeding to the eyes, and the other 'parts of the head. The ſituation of the œſo-'phagus, and the muſcles ſerving for the different 'motions of the head, were as diſtinctly obſerv-'ed. Sometimes I examined the whole or part 'of the ſeparated brain, inſtead of the nerves.

'By continuing the diſſection, and extending 'it further, I could eaſily trace the different 'parts, even to the places of inſertion in the head. 'Without the leaſt difficulty, I found the throat 'of the animal, its tongue, lips, mouth, teeth, 'the four horns, with their nerves, muſcles, and 'other appendages. The horns might be re-'tracted into the head: I had only to pull the 'end of the divided muſcles.

'Theſe, my dear correſpondent, are the moſt 'remarkable parts that preſented themſelves to

'view

'view in the divided head. I say, *the most remarkable;* for I discovered many others less 'important, of which I shall treat in my large 'work.

'Let me now ask you, if this assemblage of 'parts composing the severed head, if this as-'semblage which I have seen, and an hundred 'times seen again, is simply the integument of 'the head, or a portion of this integument, as is 'imagined by the French observers, whose expe-'riments you have communicated to me. Is it 'not the most satisfactory evidence, that I have 'been under no illusion, and that this severed 'head fully contained all the organs of which it 'is originally composed.

'I practised nearly the same method with re-'gard to the head reproduced. It would be 'superfluous to enumerate the organs com-'posing it; for I could only repeat what has 'just been said of those we discover in the origi-'nal head cut off. This decollation, indeed, fre-'quently gives place to various monstrosities of 'the parts reproduced; but the essence of my 'discovery is not affected by them.

'I have taken care to measure the severed 'head, and to compare it with that reproduced. 'Many other precautions have also been used; 'and although I omit them here, they shall be 'mentioned at large in my work.

'I flatter myſelf, that my treatiſe on the repro-
'ductions of ſnails will be ſo rich in experiments,
'and that they will be ſo amply and accurately
'deſcribed, as to convince the moſt obſtinate in-
'fidels.'

The reader may now decide whether I had any foundation for reproaching the exceſs of M. Adanſon's pyrrhoniſm concerning the diſcovery on ſnails. It is truly moſt ſingular, that he perſiſts in his doubts, after the peruſal of a letter ſo ſtrictly demonſtrative as that I have juſt tranſcribed. How many phyſical facts are admitted by philoſophers, and by M. Adanſon himſelf, which are no better eſtabliſhed than thoſe of which we treat. Shall I ſay more? M. Adanſon ſtill retained his doubts 9 October 1779, as I learned from himſelf during a viſit he then paid me on a journey for the recovery of his health. At that time, I had no ſnails in full reproduction, but I had the ſatisfaction of convincing him, by the teſtimony of his own eyes, of the reality of the prodigies preſented by the reproduction of the members of the water newt. I ſhowed him newts in various ſtages of reproduction; I ſhowed him arms, hands, thighs, legs, feet, perfectly well formed. He yielded to ſo many accumulated proofs; and was convinced that what he had erroneouſly ſuppoſed ſimple ſtumps were actually real members which would be completely regenerated.

I

I now return to my experiments on the reproduction of the head of ſnails. Thoſe whoſe progreſs I mentioned in the firſt part of this Treatiſe died before finiſhing the reparation of the head. They became much emaciated, and aſſumed a tranſparence which is unnatural to ſnails. One, whoſe anterior part is repreſented, fig. 12. reproduced a large horn only, about a line in length, but much thicker than a large horn beginning to protrude, fig. 15. c. This ſingular horn, which ſeemed to be formed like a kind of ſpindle, had two eyes, fig. 12, *o*, *o*, very diſtinct, and each with its optic nerve. The whole is magnified, fig. 15: the part reproduced, which is always lighter than the original fleſh, is indicated by the ſlight ſhading. On cloſe examination of the horn, it was immediately diſcovered to be formed by the union of two horns, which were as if ingrafted on each other. There was no mark of a mouth, nor were the ſmall horns perceptible in this ſnail: therefore, how can there be any doubt of complete decapitation (1)?

In ſpring 1778, I reſumed theſe experiments on different ſpecies of ſnails. Their different reproductions preſented varieties ſimilar or analogous to thoſe that I had obſerved in the ſnails decapitated the preceding year. One among them reſembled that which is juſt mentioned. Two

(1) There is here ſome inaccuracy in the original, which I am unable to correct.—T.

eyes could be very distinctly seen in the left horn of a snail that had begun to reproduce the mouth and the two large horns, fig. 16, *c*, *c*. The anterior part is here represented larger than life.

During the whole course of the year 1778, I continued to attend my snails. The progress of reproduction was very unequal as usual: and none completely repaired the head.

I decapitated twenty-four snails of the same species, 26 May 1780, and confined them in vessels after the operation. Several attained the summit of the vessels, and attached themselves to the sides, or to the paper covering over the mouth. The greater part shut up the shell with a very thin operculum at different degrees of depth within.

Several of the decapitated snails being immerged in water, that I might judge of the state of reproduction, 9 September 1780: two exhibited a remarkable monstrosity. One had a single large horn only, very like that of fig. 15. and was evidently formed by the union of two horns. Two small shining black eyes were at the extremity: each provided with an optic nerve perfectly visible through the transparent flesh. This monstrous horn, which was about a line long, appeared thicker in proportion than that of fig. 15. It greatly resembled a spindle, being cut even at the extremity, and all nearly of the same thickness. But it was different from the other by a more important

portant diſtinction. A little under the eye, on the left ſide, was ſeen a very minute tubercle, which ſeemed to be a ſecond horn growing out of the large one. I ſought in vain for the parts conſtituting a mouth in this ſnail. No veſtiges of it could be diſcovered, nor was there the ſmalleſt indication of the two ſmall horns.

The ſecond ſnail exhibited another kind of monſtroſity. Only one large horn had protruded; but at the extremity I thought I could diſcern three black eyes, which were ſo cloſe, that they ſeemed confounded together, fig. 17, 18: the figures are magnified. On the upper part of the horn were very diſtinctly obſerved three parallel optic nerves, only one of which proceeded to the three minute eyes. Under this horn, and at a little diſtance from the origin, a very ſmall one was diſcovered, apparently firſt beginning to expand. Here, as before, there was no viſible indication of the parts forming the mouth.

I examined the two ſnails again on the 25 of October, both with the naked eye and a magnifier. Reproduction had made very ſenſible progreſs. Only eight of the whole ſnails were now alive, and all of that ſpecies with a yellow or yellowiſh ſhell. The other ſixteen had periſhed, ſome ſooner, ſome later.

The ſix ſnails, whoſe reproduction I had not yet examined, were immerſed in water. Moſt of

them had made but little progreſs, and ſhewed only the origin of a ſingle horn: Two eye-horns had protruded in one alone, at leaſt a line and a third in length, and the optic nerves ſo large, or viſible, that they ſeemed to darken the greateſt part of the horns; but the ſmall ones, the lips, and the other parts of the mouth, did not yet appear. This ſnail obſtinately remained in the ſhell, though immerſed in water more than two hours. Suſpecting its death, I had taken it out; and, not before an interval of ſeveral hours, was I agreeably ſurpriſed, by ſeeing it proceed from its ſhell of its own accord, and diſplay its new productions before me.

In another Memoir, I ſhall give the hiſtory of theſe eight ſnails.

After experiments on ſnails of the ſmalleſt ſpecies, it was proper to make them on the largeſt. This I began to execute, 24 May 1780, on twelve of the largeſt that are found with us. We may judge of their ſize, compared with the ſnails which have hitherto been the ſubject of this and the preceding memoir, when the diameter of the mouth of the ſhell of the latter was at leaſt nineteen lines, and that of others, only four or five.

In a few weeks, half my ſnails died, and exhaled an exceſſively fœtid odour. On 13 Auguſt, I immerſed the ſix ſurviving. They proceeded from their ſhells; and I ſaw that the immenſe

menſe wound was perfectly cicatriſed, but no indications of reproduction were perceptible.

Towards the middle of October, other two ſnails died. On the 18, the four remaining were immerſed in water, where they remained more than three hours without appearing. With my nail, I ſcratched the laſt volute of the ſpiral, but in vain. All my endeavours were fruitleſs, and the ſnails obſtinately concealed the anterior part; they were then taken out, and confined in their veſſels. I will ingenuouſly acknowledge, that I had little expectation that the ſnails had made any reproductions. What, then, was my ſurpriſe, when they proceeded from their ſhells next morning, of their own accord, and exhibited unequivocal evidence of reproduction, and even of reproduction conſiderably advanced. One, in beginning to repair the head, had two large horns about a line in length, fig. 19. The left, *c*, which was thicker than the other, had two diſtinct black eyes at the extremity. The right horn, on the contrary, ſeemed to have none. No regeneration of the ſmall horns, or mouth, could yet be diſcovered.

Another ſnail had likewiſe begun to reproduce two large horns, nearly of the ſame ſize as the preceding, but the right horn was monſtrous; it apparently terminated by three ſmall ſoft points, fig. 20.

The reproduction of a third head was announced by four or five ſhort protuberances, where the large left horn, bearing three black points, or very minute eyes, was alone viſible. The whole is magnified, fig. 21; the three eyes, *a*, *b*, *c*. Two, inſtead of being at the extremity of the horn, were on the ſide; one, *a*, more evident than the other, *b*, and the third was a little below the other two. It was impoſſible to diſcern the optic nerves through the fleſh, for the reproduction was yet in an early ſtage. Under the horn were two protuberances, *p*, the nature of which could not be known; but I ſhall continue my obſervations on theſe large ſnails, and give the ſequel in another Memoir.

Engd by Lizars

MEMOIRS

ON THE

REPRODUCTION OF THE MEMBERS

OF THE

WATER NEWT,

BY

CHARLES BONNET.

MEMOIR I.

When the prodigies of the celebrated polypus aſtoniſhed the world, M. de Reaumur ventured to predict to the Academy of Sciences, that it would not be very long before many other animals were diſcovered, which ſhould preſent the ſame,

ſame, or analogous wonders (1). I was the firſt who had the good fortune to verify this prediction, and it was ſoon confirmed by the moſt celebrated obſervers (2). Various ſpecies of freſh-water worms, earth-worms, ſea-nettles, and ſea-ſtars, reproduced like the polypus, from ſections (3). Theſe diſcoveries were an immenſe addition to the riches of organic philoſophy, and an inexhauſtible ſource of meditation for the philoſopher. But we had not then attained their limits. New prodigies, perhaps more wonderful ſtill, were to be unveiled to naturaliſts; and to the illuſtrious Abbé Spallanzani was the diſcovery reſerved. It is already evident, that I particularly allude to the regeneration of the head of the ſnail, and the members of water newts. People have doubted, nay, they yet continue to doubt, theſe beautiful diſcoveries, and additional confirmations ſeem to be deſired by the impartial public. I have begun to give ſome in my Memoirs on the reproduced head of ſnails; and there, in my opinion, is the reality of that reproduction

(1) In the year 1740, *Memoires ſur les Polypes*, by M. Trembley. Leyden, 1744, 4to.

(2) In 1741, *Traite d'Inſectologie*, Part 2. Paris, 1745, 8vo.

(3) Reaumur, *Memoires ſur les Inſectes*, T. 6. Preface, page 49. Edit. 4to.

reproduction ascertained. In a second treatise, I proceed to relate recent experiments on the reproductions of water newts; and it will be seen that the renowned Reggian naturalist's discovery here, is no less certain than that of the head of the snail.

It is unnecessary, in this place, to describe the newts which were the subject of my experiments; they were exactly the same as those described in M. de Bomare's *Dictionaire d'Histoire Naturelle*, which is in the hands of all the world. A newt of full size is represented, plate 9. fig. 1. These animals are of a deep brown colour, covered with round or elliptical tints, almost black; the under part of the belly is dark yellow, and covered with black spots, and the skin of the sides shagreened with whitish or yellowish tubercles. Young newts are different, being of a yellowish green colour, with light brown specks, or minute lines. But I again repeat, that it is not meant to describe the animal, but only to give an idea of the wonderful reproductions which I have beheld.

I. *Method of preserving newts.*—The newts are kept in large glass vessels, full of fresh water, which is renewed at least twice a week; for it becomes turbid in a few days, and it seems injurious when too long of being changed. Only one is put in each vessel. From time to time, they

they riſe to the ſurface for reſpiration, and, after diſcharging ſeveral large air bubbles, immediately dive to the bottom. Theſe little quadrupeds are perfectly innocent, and may be handled with ſafety. I have often held them without the ſmalleſt attempt to bite. I have even mutilated them in my hand with impunity. This remark is neceſſary, becauſe there is a prejudice general among country people, that newts are very dangerous animals (1).

II. *Food.*—The newt is carnivorous; it ſeems to care for living inſects only, and is induced by the motions of the prey to ſeize it. In this it reſembles ſpiders and ant-lions, which refuſe to touch dead inſects.

Theſe animals can ſupport the want of food very long. Some of mine have lived two months without it. Sig. Spallanzani had remarked the ſame; and obſerved that, although long deprived of nutriment, they reproduced their members equally well as thoſe plentifully ſupplied with ſuſtenance.

In my opinion, no inſect is better adapted for food than the earth worm: we may almoſt affirm, that nature has purpoſely prepared it for feeding various animals. There are few places where worms do not abound; and as they may be

(1) M. de Maupertuis firſt proved that newts are perfectly innocent. *Mem. de l'Academ. de Paris*, 1727.

be divided into various pieces, and ſtill move, they are admirably ſuited for the food of newts. Another advantage attends them, which is the property of remaining ſeveral days alive in water; and it is their motion that particularly excites the appetite of newts. A worm preſented on the end of a pencil, or dropped into the water, is ſeized with a ſudden motion of the animal's jaws, and ſwallowed alive, with gentle ſhocks of the whole body, and eſpecially of the anterior part. A conſiderable time is occupied in ſwallowing the worm, if it is large; and it ſometimes remains two or three minutes in the newt's mouth, folding in various ſhapes while life remains. The portion hanging out very much reſembles a long thick tongue; and its numerous contorſions exhibit a moſt ſingular ſpectacle.

My newts have always appeared to ſwallow their prey without maſtication; however, they have a number of very minute teeth, which undoubtedly are not totally uſeleſs; and they ſerve to retain the prey, if attempting to eſcape.

A large worm, ſeized by the middle, is ſeldom ſwallowed in the ſame poſition, becauſe it is too large if doubled in the mouth; and the newt gradually ſhakes it out, until it can ſeize one of the extremities; which being accompliſhed, the worm is ſoon devoured. However I have obſerved a large one ſwallow a worm taken by the middle,

middle, without ſeizing an extremity; but a quarter of an hour was occupied in the meal.

Though newts are provided with flexible, jointed fingers, it is remarkable that they make no uſe of the hand, either to ſeize their prey, to convey it to the mouth, or retain it there. Yet I have never ſeen it done; they never ſeemed to uſe their hands for this, or any thing elſe indeed, but ſwimming (1).

They firſt ſeem to fix their eyes on their prey, and immediately dart upon it open mouthed; and when once ſeized, it very ſeldom eſcapes. The ſucceſſive motions of deglutition are very ſenſible; it is performed by gentle ſhocks, as we have juſt remarked. I have not obſerved newts ſearch for prey; they take it when it occurs in their way, or is juſt beſide them. A newt having devoured a large earth worm, I ſupplied it with another above four inches long, and thick in proportion. It immediately ſwallowed the whole, except a line or two that hung out of the mouth; but the worm was ſoon thrown up, and the ſame repeated twice, yet the worm ſtill lived.

It

(1) Neither do toads, which have alſo articulated fingers, make any uſe of them. However, I have ſeen one ſmall ſpecies, which profited by their aſſiſtance, to retain the prey while devouring it.

It might be ſuppoſed that the amputation of the limbs is moſt painful, and that the animals would ſuffer long and ſeverely from it: however, one of my obſervations apparently infers the reverſe. I cut the left hand and the right foot off a large newt; and a ſtream of blood, as thick as a hog's briſtle, continued ſpouting out nearly two minutes without intermiſſion. Not only did the animal ſeem not in the leaſt enfeebled by the loſs of blood, but, in ſcarcely a quarter of an hour, to my great ſurpriſe, it ſwallowed two earth worms.

Sometimes worms are devoured entire, notwithſtanding all their exertions to eſcape. They twine round the neck of the newt like a ſerpent: every moment they become ſhorter, and gradually diſappear, according to the portion which enters the body. Thus have I ſeen a newt ſwallow a worm, more than ſix inches long, in leſs than five minutes.

III. *Spoliation of newts.*—It cannot properly be ſaid that newts change their ſkin, for it is only the epidermis which is thrown off (1); at leaſt, the ſpoil is ſo thin and tranſparent, that it ſeems to correſpond with an epidermis only. It is of a whitiſh

(1) M. de Fay has already remarked this fact in his curious memoir on newts, to which I refer the reader, *Mem. de l'Acad. de Paris*, 1729.

whitiſh colour, and reſembles the fineſt gauze; indeed it is almoſt as fine as a ſpider's web. What poets have figured of phantoms may be applied to the ſpoil of newts. The whole body appears: we ſee the hands, fingers, feet and tail, but all in a ſhade floating in the water.

When the period of change approaches, the fine ſkin is obſerved detaching from the body. The head firſt loſes it; then the reſt of the anterior part; next the middle, and the poſterior part. Sometimes the ſpoil, caſt by the head, forms like a gauze collar or cravat around the neck; or it is adjuſted on the head, like a capuchin or head dreſs.

The commencement of ſeparation, from the back and belly, is diſcovered by viewing the newt obliquely from one ſide, in a ſtrong light. The ſkin of the belly is further detached, becauſe it falls down by its own weight.

Approaching ſpoliation is recogniſed by conſpicuous and unequivocal ſymptoms. The back, viewed obliquely, appears whitiſh, and as if covered with a ſpider's web. This is the effect of the ſpoil beginning to ſeparate. If cloſely examined with the naked eye, or a magnifier of ſmall power, it ſeems compoſed of minute ſcales covering the calloſities or tubercles, which ſhagreen the body of the newt. But, when examined with more attention, and in a favourable light,

light, this epidermis is difcovered to be a beautiful reticulation, the meſhes of which are viſible to the naked eye.

Many obſervations could be made on the texture of this delicate membrane; and theſe might greatly tend to elucidate the nature and origin of the epidermis, which, notwithſtanding all the reſearches of phyſiologiſts, are ſo little known; and newts would afford frequent opportunities for deeply inveſtigating the point.

From particular attention to the newts in my poſſeſſion, I have obſerved, that there is not the ſmalleſt reſemblance between this operation and what is exhibited by caterpillars, and many other inſects. The ſkin is detached here and there, and often in different ſized plates; and the change is ſlow, for it occupies one or two days, and I have even known it take three. During ſpoliation, the newt continues moving about in the water, with all the uſual motions of newts that undergo none; therefore it is no diſeaſe, and it does not affect them as it does inſects. While the change is going on, the animal darts on its prey, holds and devours it.

Sometimes ſpoliation is difficult to be accompliſhed; but, in theſe caſes, the newt knows to practiſe certain manœuvres, to facilitate the operation, which I have often beheld with pleaſure. It alternately raiſes and depreſſes the right arm

and left leg, at the ſame time with gentle vibrations of the whole body. It frequently darts ſuddenly towards the ſurface of the water, and the next moment precipitates itſelf to the bottom; and theſe manœuvres I have ſeen continued above half an hour. But the ſudden exertion, in all its motions, indicated that the newt was impatient at the tedioufneſs of the change.

When moſt of the ſpoil is thrown off, and the animal, to diſengage itſelf from the reſt, rapidly riſes to the ſurface, it ſeems carried along in a cloud; for the whiteneſs, fineneſs, and ſemi-tranſparency of the ſpoil, floating around it, is no imperfect repreſentation of a cloud.

I never obſerved the fingers employed in detaching the ſpoil. Both young newts and thoſe full grown caſt ſeveral ſucceſſive ſkins: ſome of large ſize are in my poſſeſſion, that have done ſo before me. Reproducing limbs throw off the epidermis as well as the original.

I have ſeen the ſkin of the head, which formed like a collar or cravat round the neck, gradually come down the belly of a large newt that had loſt the arms, and faſten like a tight girdle.

Nothing can accurately be ſaid of the number and interval of mutations. Between the 14 of July and the 7 of September, a newt has changed its ſkin eleven times.

1 change,

1 change,	14 July.	6 change,	9 Auguſt.
2	17	7	—
3	20	8	19
4	24	9	24
5	30	10	26
		11	6 September.

Spoliation ſometimes makes a ſlight change in the colour.

IV. *Reproduction of newts.*—It is long before I arrive at that part of the hiſtory which is moſt intereſting, namely, the reproduction of the members of newts. My experiments began 5 June 1777; and, in the courſe of that and the following month, fifteen newts were mutilated, ſome very young, and the reſt full grown. All the former died, and ſeveral whoſe increment was complete. As this treatiſe is limited to the ſucceſsful experiments, thoſe that were fruitleſs ſhall be ſuppreſſed.

I ſhall denominate the anterior extremities, *arms* and *hands;* and the poſterior, *legs* and *feet:* circuitous expreſſions will thus be avoided.

On the hand are only four fingers, but there are five toes on the feet. It has already been remarked that they are well articulated, and nearly reſemble our own. They neither have nails, nor are they connected by intermediate membranes; but I repeat, that my purpoſe is to avoid deſcriptions.

EXPERIMENT I. *The right arm and left leg amputated.*—On the fixth of June, I cut the right arm and left leg off a large newt, very near the body. A ſtream of florid blood ſpouted a minute and a half from each wound; however, the veſſels ſoon cloſed, and the newt was apparently as well as thoſe unmutilated. But it will eaſily occur that it did not ſwim with equal facility.

When about a month had elapſed, I began to perceive a papilla, of a violet grey colour, near the edge of the trunk or ſection. This was the origin of a new arm and leg, which gradually increaſed; and, on the 14 of July, appeared ſuch as fig. 1, *c*, *c*, plate 9.

The two papillæ continued growing on the ſubſequent days, but more in length than thickneſs. They became minute ſtumps; and, on the firſt of Auguſt, were about two lines long. Fig. 2, 3, repreſents the natural ſize: the trunk *t*, *t*, which has grown none, and is of a much darker colour, is clearly diſtinguiſhed from the reproductions, *b*, *c*. A kind of cleft, *s*, hardly perceptible, announces the appearance of two toes, which nature labours to produce, or rather to expand, on the new foot. No cleft appears on the originating arm.

The two toes were eaſily recogniſed on the ſeventh. They were real miniatures, and truly moſt minute, fig. 5. The ſtump of the arm, nearly

nearly as it was on the first of the month, is seen fig. 4. It is now somewhat larger, but as yet there is no indication of fingers. Fig. 6. represents it on the ninth.

It is pleasing to observe the little hand fully unfolding, while only three fingers of unequal length are visible: the middle one is the longest. The arm has made no sensible progress. The new foot had four toes also of unequal length; the first and second of which are longest; the other two only begin to appear; the fourth is scarcely perceptible. One can never tire contemplating these miniatures, and admiring the wonders of the organic kingdom.

Evolution advanced every day. On the 22 of August, the regenerated members were as fig. 8, 9. They began to deepen in colour, so that the line, discriminating the old parts from the new, was no longer so conspicuous; but the black specks on the toes of unmutilated newts were still imperceptible. The difference between these toes and those of fig. 1 is evident here. Four well shaped fingers were already on the hand, fig. 8; but only four toes, equally well formed, of the five which the foot regenerates, fig. 9; and they have to acquire more size, consistence, and colour.

I continued my observations on the daily evolution of the members; and the following were

their dimenſions in length, on the 20 of September.

Old Members.		*New Members.*	
Arm,	4 lines.	Arm,	$2\frac{1}{2}$
Cubit,	$3\frac{1}{2}$	Cubit,	$2\frac{1}{2}$
Thigh,	3	Thigh,	$2\frac{2}{3}$
Leg,	4	Leg,	$2\frac{1}{4}$
Longeſt finger,	$3\frac{2}{3}$	Longeſt finger,	$1\frac{1}{2}$
Longeſt toe,	$4\frac{1}{2}$	Longeſt toe,	$1\frac{1}{3}$

Even in the beginning of October, the fifth toe of the new foot was not viſible.

EXPERIMENT II. *A newt deprived of the right arm and left hand.*—On the 12 of June, I cut the left hand and right arm off a newt: my chief object in this experiment was to verify Sig. Spallanzani's aſſertion, that nature reproduces exactly the portion amputated, which was a fact of the utmoſt importance in the theory of animal reproductions, and could not be too well eſtabliſhed.

Towards one ſide of the ſection, a little conical nipple began to appear about the 7 or 8 of July, of a violet grey colour. An incipient cleft, indiſtinctly ſeen with the naked eye, was perceptible near the middle of July: the papilla ſeemed ready to divide in two; and the cleft was the origin of two fingers.

In

In two or three days, I remarked a new cleft at the upper extremity of the papilla, which was the beginning of a new finger: the third, in its turn, appeared on the nineteenth. The conical papilla had then disappeared; and in its place was seen a small open hand with four fingers, still very minute, but quite well shaped. It is drawn from nature, such as it then was, fig. 10. That papilla, from which originated a new arm, gradually extended: at first it was similar to the papilla, *c*, fig. 1. but towards the end of July, or beginning of August, it increased so much as to resemble that of fig. 2. exactly.

On the third of August, the cone began to divide, that is, two fingers became evident, fig. 11. Close examination is required, for the division of the fingers is hardly visible, 1. the trunk *t*.

On the ninth, a hand extremely minute, but the most beautiful object imaginable, was observed at the extremity of the arm. The fingers, all of unequal length, were distinguished, the smallest being just perceptible. This is correctly designed, fig. 12. The trunk *t*, or part of the original arm, is connected to the body. It may be recognised by the brown colour, and from being covered with white points. The new arm *b* is of a lighter and uniform colour. Four fingers

of the hand, *m*, are viſible: the largeſt not above half a line in length.

The hand of the left arm had made conſiderable progreſs on the 21: it had expanded, and nearly acquired the figure peculiar to the newt's hand. The fingers alſo had extended, and become thicker in proportion. The whole hand began to colour, and brown ſpecks were diſtinguiſhable on different parts; they were more evident on the back of the hand than on the fingers. This will be better underſtood by fig. 13. than any deſcription can render it; and on comparing this with fig. 10, we obtain a more correct idea of the progreſs of evolution.

The reproduced arm is deſigned, fig. 14. as obſerved on the twenty-firſt. The hand has already aſſumed its natural ſhape, and the rapid progreſs of evolution is ſuſpending. Colouring of the arm begins near the trunk: but all the reſt is of a mixed grey and violet.

Though I have not hitherto expreſsly ſaid ſo, it will obviouſly be preſumed, that there is a kind of ſemi-tranſparency in the reproduced parts, which the original members have not. This continues long, and changes ſlowly as the reproductions colour. The tranſparency is evidently greater on the edges of the fingers than elſewhere, if examined with a magnifier, they ſeem incloſed in a fine diaphanous envelope: but nothing

thing of this is evident in the old fingers. Parts beginning to unfold naturally have a degree of tranſparence wanting in thoſe further advanced, or fully expanded, becauſe, with the progreſs of evolution, the calibre of the veſſels increaſes, which allows admiſſion to more groſs and colouring particles. Whiteneſs and tranſparence apparently conſtitute the primitive ſtate of organic bodies. It is this primitive ſtate which we deſign by the word *germ*, and which we can comprehend, when the organic whole is expanded to a certain extent. But there is here a term beyond which we cannot aſcend; for the organic whole either becomes ſo minute or ſo tranſparent, that it eſcapes all reſearch and our moſt perfect inſtruments.

The dimenſions of the old and new members, in length, were as follows, on the 2 of September.

Old Members.		*New Members.*	
Arm,	$3\frac{1}{4}$ lines.	Arm,	$2\frac{1}{3}$
Cubit,	$3\frac{1}{2}$	Cubit,	$2\frac{1}{4}$
Longeſt finger,	$1\frac{1}{2}$	Longeſt finger,	$1\frac{1}{2}$

Experiment III. *Two fingers and three toes amputated.*—I cut two fingers and three toes off a large newt, 15 July. A little elevation was perceptible on the ſection of each finger, on the

26. At the end of each trunk, a new finger appeared, 20 Auguſt. The fingers, *n*, *n*, are deſigned, fig. 15. The whole magnified is more diſtinct, fig. 16. The fingers have yet grown very little.

The five toes are magnified, fig. 17. Conſiderable part of three that had loſt about one half, is already reproduced, and the new production is at the extremity of the old portion, *a*, *a*. An irregular curve, ſeparating the two parts, is very conſpicuous here, and evinces that the old portion has grown none.

EXPERIMENT IV. *The right hand of a newt longitudinally divided, and two fingers amputated. The other hand cut tranſverſely.*—On the third of Auguſt, I made a longitudinal inciſion through the right hand of a large newt, in ſuch a manner as to ſeparate two of the fingers. At the ſame time, I amputated the other hand in the middle, by a tranſverſe ſection.

The left ſtump exhibited the uſual conical papilla on the 22. It began to divide on the 30; and three originating fingers became diſtinctly viſible.

On the ſame day, a new finger appeared on the ſide of the right hand. Two of the fingers had been left entire, *a*, *a*, fig. 18. A ſenſible eminence, *e*, on the ſection of the hand, preceded the

the origin of the new fingers, which is very evident, *r*. On the firſt of September, a ſecond finger began to grow beſide the other, and on the fourth, this hand, which had been cut longitudinally, was as it is repreſented fig. 19. The two fingers which replace thoſe amputated, *r*, *r*. On the ſame day, the new hand growing from the left arm, was nearly as fig. 10.

This experiment is attended by a moſt remarkable conſequence; that a hand, longitudinally divided, ſhould reproduce exactly the amputated part, and reproduce it both in the manner and place moſt conſonant to its natural figure and functions.

The hand was deſigned anew, 9 September, that one may judge better of the poſition of the regenerated fingers and the progreſs of their evolution, fig. 20.

Experiment V. *An oblique ſection of the hand, ſo that only one finger remained.*—It would be too precipitate to conclude, from the preceding experiment, that nature never reproduces more than the portion amputated, and ſhe herſelf would diſclaim it. Nevertheleſs, deciſive experiments prove the fact. On the 21 of Auguſt, I cut the right hand of a newt, in ſuch a manner as to leave only the firſt finger. Four very minute papillæ appeared on the edge of the ſection,

tion, 13 September. I could eaſily recogniſe them for the rudiments of four new fingers. Yet, from the danger of deception, I ſuſpended my opinion; however, as evolution ſenſibly advanced, four ſmall well ſhaped fingers aroſe, which it was impoſſible to miſtake, fig. 21. Their arrangement is irregular, for the firſt and ſecond ſeem united, and the diſtance between them is a little leſs than between the two laſt, *c*, *d*.

Behold a hand with five fingers, though nature has given the animal only four. However, it is not uncommon to meet with ſuch anomalies in theſe reproductions. Sig. Spallanzani has obſerved many, which he will deſcribe with his uſual acuteneſs and perſpicuity. It is evident, that the place where the cut is made, the mode of making it, the ſtate of the part itſelf, and of parts connected, may give birth to infinite varieties and ſingularities; and ſome will be real monſtroſities, either by defect, exceſs, or tranſpoſition.

It muſt be obſerved, that in this caſe, reproduction was not announced by one papilla only, as in the firſt two experiments, but by four very minute, and quite diſtinct, ranged in the ſame line, and in the direction in which the inſtrument had performed the operation.

EXPERIMENT

EXPERIMENT VI. *The tail of a newt amputated tranſverſely.*—Something important would have been wanting, had I neglected amputation of the tail, which is a very intricate great organic ſubſtance. It is formed of a ſeries of minute vertebræ, with arteries, veins, and nerves, and it is covered with muſcles and fleſh.

The tail of a large newt is more than two inches long, and about half an inch thick, formed like an oar, and terminated by a ſoft point. Much might be ſaid of the figure, proportions, and poſition of this organ, and with reſpect to the functions it has to exerciſe; but theſe would be details foreign to my purpoſe; I only mean to confirm what Sig. Spallanzani has advanced concerning the admirable reproduction of the members.

When the tails of large newts were amputated near the origin, I never ſucceeded in obtaining reproduction; the whole died in a certain time; and, for ſeveral weeks preceding death, a kind of whitiſh cotton mould grew on the wounds, the filaments of which were ſeveral lines in length. Nevertheleſs, I cannot think that this affected the animal's life, for I had ſeen ſimilar mould, or cottony filaments, on wounds occaſioned by amputating the arms and legs. Theſe filaments gradually diſappeared, and unequivocal ſigns of reproduction

reproduction ſoon became viſible. Thus a good obſervation was never obtained, unleſs the tail was divided about the middle, and by a ſection perpendicular to the axis. A ſtream of blood, as thick as a hog's briſtle, always ſpouted from the wound. The large veſſel, from which it flows, is ſituated near the vertebræ, and its orifice is viſible by the naked eye; it immediately cloſes; and the orifice is diſtinguiſhed by a reddiſh or browniſh point.

The tail of newts is very ſenſible, which is particularly evident in the ſlendereſt part. A portion cut off will retain life, and move whole hours; and when life ſeems entirely extinct, we have only to prick the pointed extremity, that motion may be renewed; it riſes and falls alternately, and with greater force, according to the period that has elapſed ſince the operation. The motion of this ſeparated part bears great reſemblance to that which is peculiar to certain apodal worms; it is undulatory, and evidently depends on irritability, which is extremely active in ſo muſcular an organ.

Immediately after the operation, the area of the cut exhibits a very long ellipſe; the two extremities almoſt terminating in a point. The ſmalleſt diameter is about a line acroſs, and the largeſt five or ſix. In the centre, are the vertebrae,

bræ, or blood veſſels; the reſt of the area ſeems full of ſmall oblong clear white ſubſtances, which one would ſuppoſe pieces of fat, or glands. The ſurface ſlowly contracts; the oppoſite ſides approach; the colour of the ſubſtances becomes fainter, and in a certain time, which is according to the ſeaſon, new fleſh appears, and it daily increaſes. Then we obſerve one or two croſs brown lines, occupying the middle of the new tail, which indicate the ſite of the vertebræ and the veſſels. The firſt evolution is deſigned, fig. 22; the part reproduced, *n*, is thinner and more tranſparent than the reſt; the brown line, *c*. At the extremity, is a ſmall internal cavity, *m*, very perceptible in the figure, which I have uniformly found in reproducing tails. This tail had been divided on the 11 of July; and on the 14 of Auguſt the reproduced part was about three lines and a half long, and four and a half in diameter, at the baſe.

The new portion was ten lines in length, 20 September, and ſhaped exactly like the tail of a newt. I could obſerve no difference between the motions of this regenerated tail, and thoſe of tails unmutilated. It was again deſigned, 8 October, fig. 23; the reproduced parts, *r*, *r*. The edges of the old trunk had then grown none; they are clearly diſtinguiſhed, *t*, *t*.

Thoſe

Thoſe of the regenerated part had a peculiar tranſparency, wanting in the reſt of the tail (1).

GENERAL RESULTS AND REFLECTIONS.—Other experiments have been made on newts, which confirm the firſt, but I paſs them in ſilence, to avoid repetitions and the multiplicity of details. What I have ſaid, ſeems ſufficient for my principal object, which is only to eſtabliſh the diſcovery of my illuſtrious friend the Abbé Spallanzani. When his large work appears, naturaliſts will there behold with aſtoniſhment, the various prodigies for which we are indebted to the ſingular ſagacity of this celebrated obſerver, and of which this little tract can give but a very imperfect idea. So much inferior to the ſubject do I find my own experiments, that I never ſhould have thought of publiſhing them, had not new confirmation been deſired, and had not the Abbé himſelf wiſhed my teſtimony in addition to his. I next proceed to deduce ſome general results

(1) Had not the fear of fatiguing my eyes too much deterred me, I ſhould have endeavoured to compare the new limbs with the old, by diſſection, and the ſame with reſpect to the head of the ſnail. But, in my opinion, the ſimple narrative of facts, or inſpection of the figures, are ſufficient to demonſtrate the reality of the reproductions, and the ſimilarity of the new members to the old.

refults from my experiments, which ſhall be limited to thoſe directly flowing from facts.

The firſt, which is evident, reſpects the time employed by nature in preparing for the reparation of mutilated members. In the arm polypus, and freſh water worms, which may alſo be multiplied from ſections, reproduction is immediate, and in one or two days, during ſpring or ſummer, there are ſenſible indications of regeneration. It is very late in newts, on the other hand; and only, after an interval of ſeveral weeks, do any ſymptoms of renewal appear: thus, in the newt, fig. 1. it was imperceptible till about five weeks after the operation; and a month was required for preparing the reproduction of the newt mutilated 12 June. Polypi and freſh water worms are gelatinous; they neither have bones, nor any part that can oſſify. But it is by no means the ſame with newts; for they are actually ſmall quadrupeds, and, as quadrupeds, have bones covered with muſcles and fleſh. All theſe parts exiſt in a gelatinous ſtate, previous to their appearance. This gelatine, however, unqueſtionably oppoſes more reſiſtance to the power which effects increment, than is done by that of polypi and freſh water worms, for the ſtructure is more complicated. Thus we ſhould not be aſtoniſhed at the ſlow progreſs of evolution in large newts; in this caſe, it is even more

immediate than when the newt is old, as the Abbé Spallanzani has already obſerved, and as I myſelf have ſeen. The reaſon is evident; for the younger an animal is, the more ductile or pliant are its ſolids, becauſe their fluids are more abundant. Irritability has alſo greater energy in young animals, becauſe they are more gelatinous. We have ſeen in the Memoir on the reproduction of ſnails, that a conſiderable time is neceſſary before the new part becomes perceptible.

The ſecond fact demonſtrated by my experiments, reſpects the firſt appearance of the members. A conical papilla ariſes, which, compared to a vegetable bud, we may call an animal bud. But the compariſon muſt be extended no farther, for the vegetable bud is properly only the envelope of a plantula, whereas this animal bud is the member itſelf, infinitely contracted in miniature. The fact is evinced, by attending to the progreſs of evolution. The ſummit of the papilla divides in two, and we immediately diſcover that diviſion is produced, by the ſeparation of two fingers, formerly united or confounded together in the ſame organic maſs; the like enſues with all the fingers, which ſucceſſively appear. The papilla is therefore a real hand, or foot, already formed; but the concentration, minuteneſs, and tranſparency, prevent it from being recogniſed in its original ſtate. Yet it muſt be obſerved,

ſerved, that reproduction of the tail is not accompliſhed exactly in the ſame way as that of the other members; it is not announced by a papilla riſing in the centre, but by a thin ſemi-tranſparent plate, extending over the whole, or moſt of the ſection, and very much reſembling an edge-tool in figure.

From a third fact, it ſeems to reſult, that the members replacing thoſe mutilated, are not properly *generated*, but that they originally exiſted in miniature in the great organic whole, and are only now unfolded. This we are obliged to admit, on conſidering that the animal bud is the member itſelf, already formed, and requiring nothing but ſize, ſtrength, and colour, to bring it to perfection. Therefore, it is probable that the reproduced members pre-exiſted in germs of exceſſive minuteneſs, and that all their parts were there. The ſpecies, proportion, and poſition of theſe, which I may call *reproductive* germs, regulate the kind, the manner, and the place of reproduction. Here it is needleſs to demonſtrate how unphiloſophical it would be to reſort to formations purely *mechanical*, for an explanation of theſe admirable reproductions. My method of philoſophiſing on this important ſubject is known; and it gives me great ſatisfaction, that Sig. Spallanzani's numerous and beautiful experiments on infuſion animalcula and animal reproduction,

confirm the principles which, above thirty years ago, I had adopted, concerning the origin and evolution of organic beings. The work, which that excellent philosopher has lately published, teaches us what our real sentiments should be of the *vegetative powers* and *organic molecules* of celebrated modern Epigenists. Not only has he rigorously demonstrated the falsity of their hypotheses, by infinite various and correct experiments, but he has discovered the cause of error, and shewn what the authors should do to avoid it. I cannot too earnestly exhort naturalists to read and reflect on this beautiful work, which I may justly consider one of the most perfect models of the art of observation (1).

In the fourth place, it is evident that nature in general reproduces exactly the portion amputated; thus, when a hand is cut off, nature renews a hand only; if an arm is amputated, she regenerates one with all its parts. But, as already remarked, there are various exceptions to this law, and the fifth experiment is a striking instance of it. Sig. Spallanzani will describe many more singular, which his long experiments have given him an opportunity to observe. It may well be conceived, that it is not difficult to

(1) Tracts on the Natural history of animals and vegetables.

to produce numerous artificial kinds of monſtroſity, and that theſe may throw much light on the theory of animal reproductions. Certainly theſe wonderful operations are regulated by laws which depend on the nature and relation of various organic wholes, and it is the inveſtigation of ſuch laws that ſhould chiefly occupy the philoſophic naturaliſt. Some will be found more or leſs general, or ſpecial; ſome ſubordinate to others, and all to a more comprehenſive law, that regulates the whole organic ſyſtem. Here nothing happens by chance; all has been weighed, calculated, and combined, with reſpect to poſſible occurrences, and there is not the ſmalleſt alimentary atom in this wonderful ſyſtem of organs, without its proportions, its motion, its place, and purpoſe. Thus, what we denominate anomalous, or monſtrous, is the neceſſary conſequence of the admirable laws which govern the organic world, and of courſe a confirmation that ſuch laws exiſt.

There is a fifth reſult to be obſerved. When only a hand is amputated, what ſucceeds it is at firſt much larger than what unfolds at the extremity of a new arm. This is evident by comparing fig. 10 and 12. In the germ prepared for reproducing an arm and all its accompaniments, the integrant parts of the hand ſhould be ſmaller than in the germ, which contains no more than the reparatory elements of a hand;

at leaſt, obſervations ſeem to indicate this, for the conical papilla preceding an arm, is no larger than that preceding a hand. Probably the body of a newt includes a number of germs of different kinds, appropriated to the various reproductions that are to enſue, and each germ is placed in the ſituation and manner beſt correſponding to its evolution. But I ſhould refer to what is ſaid on the ſubject, Part 9. 10, of *La Palingeneſie*.

A ſixth reſult ariſes from the evolution of the fingers and toes. It is not effected in the ſame proportion as evolution of the arm and leg. Now, when I write this, the tenth of October, the new arm and leg of the newt, which was mutilated 6 June, have nearly attained the ſize of the original members, while the regenerated fingers and toes have not acquired half their natural ſize; yet they are perfectly well formed, and execute all the functions peculiar to theſe parts.

The ſeventh and laſt reſult is preſented by the trunk of the mutilated members. While the reproducing part unfolded, I never obſerved any elongation of the trunk. In this reſpect, there is a coincidence with the ſtate of the trunk of earth worms, and of thoſe freſh water worms which I multiplied by ſections thirty-ſix years ago; and the truth of this obſervation may be judged of, by inſpecting fig. 2. 3. 10. 12. 17. 22. 23. plate 9. The ſame enſues in regeneration

of

Fig. 1.
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of the ſnail. When the fibres of an organiſed body are indurated to a certain degree, they are no longer ſuſceptible of extenſion; there is a term beyond which the elements of the ſolids cannot glide along each other. This reſult affords an opportunity of obſerving, that it evidently concurs in proving the part to be actually a new organic whole, which expands on the old one, and is in a manner ingrafted on it. In another treatiſe, I have inſiſted much on the point.

Perhaps this Memoir may be followed by another, containing the ſequel of my experiments. I intend to diverſify them greatly, and thus give birth to new facts by new combinations. But it muſt be remembered, that all which it is in my power to do, will even be infinitely inferior to what the public may expect from the learning, ability, and induſtry of the celebrated Reggian philoſopher. No naturaliſt has equally enriched the hiſtory, ſo new and intereſting, of *Infuſion Animalcula* and *Animal Reproductions*; and I may predict, that his account of the generation of animals and vegetables, will not be leſs valuable. How impatiently would the admirers of natural hiſtory expect theſe new fruits of the indefatigable obſerver's labour, did they know, as I do, the important and unexpected truths which they contain.

MEMOIR II.

My obfervations were continued on the newts, whofe hiftory I have given in the preceding memoir. That which was mutilated on the fifth of June 1777, and the fubject of my firft experiment, died on the tenth of December; but what occafioned its death I am ignorant. The limbs increafed and coloured: the fifth toe was wanting; indeed I did not expect it after the beginning of October.

The newt, mutilated 3 Auguft, which is mentioned in the fourth experiment, paffed the winter fafely in my apartment, and lived until 8 April 1778: it then died, probably from neglect to renew the water. The two new fingers, longitudinally divided, had not acquired above half their natural length.

The newt of the fixth experiment, whofe tail was mutilated 11 July, fhared the fame fate as the preceding. It died towards the middle of November. The tail had extended very much; and the new part could fcarcely be diftinguifhed from the old, except by the lighter colour.

I had

I had more reaſon to regret loſing the ſubject of my fifth experiment, that is, the newt which had loſt three fingers and reproduced four. The monſtroſity rendered it precious. It had been carefully preſerved during winter, and was in perfect health in the following ſpring; therefore I cannot tell from what cauſe it died. Sig. Spallanzani has proved, that worms are found in theſe little quadrupeds; perhaps their numbers might occaſion death. Whatever it was, the four new fingers had grown conſiderably, and become dark coloured; but their arrangement was equally irregular as at firſt.

Here I may remark, that it is unneceſſary to change the water as often in winter as during ſummer. That in a veſſel, containing one large newt, has remained eight days pure in winter; while it has become turbid in two or three, and ſometimes ſooner, during ſummer. Newts then perſpire, and evacuate more copiouſly, and are alſo more voracious. The ejecta are grey filaments, or flocks, floating in the water (1).

It is incredible how long the new fingers require to attain the ſize of the old. I have had newts, whoſe fingers, in thirteen months and more,

(1) Newts frequently void ſmall round or oval ſubſtances, of a brown colour, but of very little conſiſtence. Some are at leaſt as large as a pea.

more, were not as large as thoſe of unmutilated members.

In general I have ſuppoſed it was otherwiſe with the arm and thigh reproduced, and with the cubit and leg; for the former, in both caſes, ſooner acquire their proper ſize, as is already remarked in the ſixth reſult of the preceding memoir. The like ſucceeds with the tail. But all theſe reproductions are very ſlow, when compared with thoſe of polypi and freſh water worms; reaſons for which are aſſigned in the firſt memoir.

I ſhall now proceed to my new experiments on quadrupeds, ſo worthy of the attention of naturaliſts.

EXPERIMENT I. *Whether reproduced members poſſeſs the ſame ſources of reproduction as thoſe amputated.*—I cut the left arm and right thigh off a large newt, 2 June 1778. In the beginning of July, a new arm and thigh began to reproduce. They were ſtill in miniature, but the fingers and toes ſufficiently formed, and very diſtinct. They were nearly as thoſe of fig. 6, 7, plate 9.

On the eleventh of July, I made an experiment, which was moſt important in the theory of animal reproductions. The object was to diſcover whether the members now reproducing, which in reality were miniatures, contained the ſame ſources of reparation as the original; that

is,

is, whether there were, in new limbs, germs containing members in miniature, similar to those amputated. With this view, I cut off the regenerated hand and foot.

At the extremity of the reproduced leg, on the 21, appeared two new toes extremely minute, but easily recognisable by the naked eye: and, on the 24, an originating hand, with three well shaped fingers, appeared at the extremity of the new arm.

The foot, now reproduced, exhibited four very distinct toes. Both these and the fingers were yet only one fourth, or one third, of a line long.

Therefore it is proved, by this first experiment, that the reproduced limbs of a newt can make new productions, in the same manner as the old ones can, and give birth to members which, in their essential parts, resemble those amputated, and are different only in size, consistence, and colour: for, as was remarked in my former memoir, the new members are of more delicate texture, and of a much lighter colour than the old.

It was undoubtedly most interesting to ascertain how far the resources of nature extended; and whether, after several successive mutilations of the reproduced member, a new one would still regenerate.

On

On the 31 of July, for the second time, I cut off the reproduced hand and foot of my newt; the fingers and toes being then about a line long.

Two new fingers and toes appeared at the extremity of the limbs, 13 August; therefore a hand and foot had begun to regenerate. On the 15, there were three fingers and toes already well formed, though very small.

Both the hand and foot seemed quite repaired on the 24, though still of extreme minuteness. All the fingers had grown, but only four toes. And it may now be observed, that the appearance of the fifth toe is constantly later; often it does not unfold.

On the same day, 24 August, I amputated the reproduced hand and arm the third time; and, on the 13 of October, performed the fourth operation: the limbs being then in the same state as those mutilated by the third amputation.

Thus it is fully established, that every member, successively reproduced, contains new sources of reparation; and that they are actually existing, though the member is extremely minute.

From these successive mutilations of reproduced members, I have thought the extremity of the leg and arm became a little thicker than usual, as if from a reflux of the nutritive fluids into the extremity, by such repeated amputations.

This

This ſeaſon was particularly favourable to my experiments, being always dry and warm. A mercurial thermometer, in the ſhade, completely iſolated, on a large terrace, ſtood at 90° and 93°, on the 14 and 15 of Auguſt. Moſt of ſummer it ſtood between 79° and 81°; and the temperature of the apartment, where the newt was kept, differed very little from that of the open air.

Experiment II. When a large newt was treated as has juſt been related, I made another experiment on one of ſimilar ſize, to obtain comparative reſults.

The left arm and thigh were ſevered 2 June 1778. Reproduction of new members commenced in the beginning of July: two well ſhaped toes were then on the foot. On the 11, new limbs had replaced the old; they ſeemed completely repaired: ſtill they were only miniatures of moſt delicate texture. This day I amputated the reproduced hand and foot.

A new foot, with two diſtinct toes, was perceptible on the 22; and three were viſible on the 24. But the new hand had not appeared; at leaſt there was no evidence of originating fingers. The thermometer now ſtood about 84°. However, a new hand, with three perfect fingers, was ſeen on the 29.

The

The reproduced hand and foot being a full line long on the 31, I then cut them off. Both appeared again 15 Auguſt, with three well ſhaped fingers and toes. On the 24, the hand had acquired its four fingers, and the foot five toes, all viſible, though exceſſively ſmall.

I then cut off the hand and foot for the third time. The fingers and toes were a full line long 13 October; four of each appeared, but the fifth toe was yet imperceptible.

Next I performed a fourth amputation: it alſo was followed by reproductions. Various occupations having interrupted me, a fifth amputation was not made before 26 Auguſt 1779.

The longeſt finger was then about one line and a third, the longeſt toe one and a half in length, deep coloured and very ſlender. The hand had four fingers; the firſt and fourth imperfect. The foot had only three toes, more diſtant from each other than uſual. Both the fingers were as imperfect 30 October 1780: the fourth ſcarcely viſible, and conſiſting only of a ſhort point; and no more than three toes on the foot. The newt had then diminiſhed greatly in ſize, and was very brown. It ate little, and ſeldom: it remained long at the ſurface, unable to get to the bottom of the water; and its belly was almoſt always very much inflated.

Theſe

Theſe are two experiments, therefore, which concur in eſtabliſhing the ſame fact, namely, that the reproduced members of a newt, though ſtill in miniature, are equally provided with reparatory germs as the old limbs; and that they begin to unfold after the new members are cut off.

Experiment III. *A foot cut obliquely, and a hand longitudinally.*—Experiments ſuch as I relate cannot be ſufficiently diverſified. The place and mode of ſection muſt have a certain influence on the place and mode of reproduction. Undoubtedly the germs, deſtined for theſe prodigies, cannot be fortuitouſly diſſeminated in the members. It is much more philoſophical to believe, that they are arranged in an order which we ſhould admire, if our moſt powerful magnifiers could bring them into view. But we are ſtill incapable of penetrating the ſecret organization of an animal: all that is permitted us to do, is reduced to a few experiments on what the mind can comprehend. Chiefly with this view, have I amputated the limbs of newts in different ways; that is, ſometimes tranſverſely, ſometimes more or leſs obliquely, and ſometimes longitudinally. And the detail of an experiment is now to be given, executed in the ſecond and third manner, as enough has been ſaid of the firſt.

On

On the 29 of January 1778, I cut the left foot of a large newt obliquely, leaving only the firſt toe. The four amputated toes were renewed 5 June; but not exactly in the ſame poſition as when the ſection was perpendicular to the length. The extremity of the foot was a little ſwelled, fig. 1, plate 10: the toe preſerved, 1; thoſe reproduced, 2, 3, 4, 5; the ſwelling of the foot, *r*. To judge of the difference ariſing from the two modes of operation, this figure muſt be compared with fig. 9, plate 9.

On the ſame day, I endeavoured to divide the left hand of the animal longitudinally through the middle, extending the ſection as far as the arm. Mould appeared on the wound in a few days, and made rapid progreſs. Part of the member at laſt mortified, and three fingers diſappeared.

The arm and hand were, as repreſented fig. 2, on the 5 of June: the finger preſerved, 1; thoſe reproduced, 2, 3, 4, which are not quite in the ſame poſition as obſerved in a tranſverſe amputation; the extremity, *r*, is ſenſibly ſwelled.

It ſhould be obſerved, that in this experiment, as well as in almoſt the whole related, nature reproduced an equal number of parts as thoſe amputated; which is a fact well deſerving conſideration.

EXPERIMENT

EXPERIMENT IV. *The limbs of a newt cut longitudinally through the middle.*—I divided the right hand and foot of a newt longitudinally, 16 June 1778. In a few days, the wounds were covered with whitiſh mould, which grew rapidly. It gradually became thicker and longer, and, as I foreſaw, announced the loſs of the limbs. I removed it ſeveral times with a pencil, but it conſtantly returned. This ſingular production requires more profound inveſtigation than I was able to beſtow on it. It evidently conſiſted of extremely delicate filaments, ſimilar to what characteriſe the mould originating on humid animal and vegetable ſubſtances. Sometimes the filaments grew half an inch long, and even more. Something ſimilar occurred in the freſh water worms, multiplied by ſections, in 1741, 1742; and I then conſidered the mould as a precurſor of gangrene. While obſerving it on the newts, the ſame idea recurred; and I thought of anointing the wounds with a pencil dipped in a ſolution of bark, but I cannot affirm that this was more efficacious than the former plan reſorted to. Whatever was the caſe, notwithſtanding all my care, the third toe and ſecond finger fell off about the end of June. Longitudinal diviſions certainly occaſion the greateſt diſorder in the parts, and particularly in the veſſels;

at leaſt, the wound is very large, and a great internal ſurface laid open.

The parts of each member, ſeparated by the operation, were fully united, and the wounds cicatriſed 26 July. The cicatrices had even entirely diſappeared.

I now obſerved a ſmall ſemi-tranſparent grey button in the middle of the foot, which announced the finger deſtined to replace that which had fallen off. Fig. 3, *n*, the button or papilla. The foot is a little larger than when in the natural ſtate; and the new toe originates exactly in the proper poſition for replacing that which was deſtroyed by gangrene.

The divided hand is repreſented as it was 2 July, fig. 4. There is no indication of a cicatrice, all is ſo perfectly healed. The new production to unfold is ſtill imperceptible.

Againſt Auguſt 11, the new toe had made great progreſs; and the papilla had aſſumed a figure which rendered it impoſſible to be miſtaken for a real toe, fig. 5, *n*.

The hand is as it was the ſame day, fig. 6: the origin of the finger to replace the loſt one, *n*. But the newt died on the 14, without my being able to diſcover the cauſe of its death.

Experiment V.—On the 27 of July, I repeated the preceding experiment on the left hand

and

and foot of a large newt. Mould did not fail to vegetate on the wounds, and, in three days, was so abundant that the members seemed endangered. However, I accomplished the removal of great part with a pencil: but, alarmed lest I should be unable to preserve either half of the members, I resolved to amputate the moulding part by a transverse section.

In several weeks, two papillæ, announcing the reproduction of two fingers, arose on the transverse section; next arose another, on a similar section of the foot, which indicated the origin of a new toe. The enormous wounds cicatrised so well, that no vestige of them remained.

The papillæ continued growing during the subsequent weeks; and, on the 15 of September, the hand had two fingers about a line and a half long. Their position was such as to give the hand its natural shape, but they were of a lighter colour than the old fingers.

The new toe was a little longer than the largest finger, being about two lines in length. The fifth did not appear, nor was it visible on the 26 of October.

Experiment VI. *A newt deprived of the third and fourth finger of each hand, and the five toes of the left foot.*—Among the beautiful phenomena which the reproductions of newts exhibit,

one of the moſt ſtriking is the conſtancy with which nature reproduces the ſame number of parts as thoſe taken away. This is particularly true with regard to the hands. If three fingers are amputated, other three will be reproduced: if two are ſevered, only two will originate. I have already obſerved, that the conſequence is leſs certain in mutilations of the foot; and it frequently occurs, that four toes are regenerated when five are amputated. But I can form no conjecture on the cauſe of this irregularity.

I cut the third and fourth fingers off each hand of a large newt, and the five toes off the left foot, 31 July 1778. All the fingers and toes were reproduced, and grew as uſual from minute papillæ: they gradually extended, and aſſumed the figure and proportions peculiar to fingers and toes.

On the 27 of October, the left hand only was deſigned, but not the right, becauſe it was quite ſimilar, fig. 7, *n*, *n*, the new fingers; although theſe were well proportioned and regular, they had not attained half the natural ſize.

It was different with the toes. The three laſt, indeed, *c*, *d*, *e*, fig. 8, grew in their natural place; but the other two, *a*, *b*, came above the reſt, ſo that the ſecond was above the third inſtead of being by its ſide. The figure of the foot was very much changed; it was larger or ſwelled. I mention

mention theſe monſtroſities, becauſe they are not indifferent in the hiſtory of reproductions.

EXPERIMENT VII. *The reproductions of newts are retarded by cold.*—It is well known how much the evolution of all organized bodies is promoted by heat. Animals vegetate like plants, and heat powerfully advances their vegetation.

Thirty-ſeven years ago, experiments on polypi, and different ſpecies of freſh water worms, demonſtrated that the reproduction of theſe ſingular animals was greatly retarded by cold. If the mutilation was towards the end of autumn, and particularly if in winter, reproduction required ſeveral weeks; and, on the other hand, it would be complete in a few days, in ſummer. My experiments on newts confirmed the fact in a moſt conſpicuous manner, as will immediately appear.

On the 6 of September 1777, I cut the arm and thigh off a large newt, very near the body, and kept it in an apartment without fire until the month of January. Being then afraid of injury from the cold, I carried it to my own chamber, along with other newts, which were the ſubject of various experiments. All paſſed the winter ſucceſsfully. The temperature of the chamber was commonly about 43° or 45°; but it frequently diminiſhed 5° or 6° during night.

The new members were no longer on the 6 of March than they would have been in fix weeks, or two months, in ſummer, fig. 9, 10, the originating arm *b*, the thigh *c*. Thus they were only two ſtumps, after an interval of fix months.

Two fingers were growing at the extremity of the arm, 9 April, and three well ſhaped toes perceptible at the extremity of the leg, *b*, *c*, fig. 11, 12. The fourth toe, juſt begun to appear, fig. 12, correſponds with fig. 7 of the firſt memoir.

In the following month the newt died, which interrupted my obſervations on the courſe of reproduction.

EXPERIMENT VIII. *The tail of a newt divided longitudinally.*—We have ſeen the effect of a tranſverſe ſection in the fixth experiment of the preceding Memoir. It has already been remarked, that the tail is formed of a ſeries of ſmall vertebræ or oſſicles, and has blood veſſels, nerves, fleſh and muſcles. Therefore this is a very complicated member; and we immediately comprehend how admirable its reproduction from a tranſverſe ſection muſt be: for not only are the veſſels, nerves, and muſcles then reproduced, but likewiſe oſſeous parts of intricate ſtructure articulated together, and playing on each other. Yet how many parts, both ſoft and hard, of much more

more complex ſtructure, are in the ſame manner reproduced in the reſt of the limbs with equal facility and regularity. Thus it may eaſily be conceived, if the ſkin of the tail is ſlaſhed, if various deep inciſions are made into it, and ſlices of different ſize cut out along the vertebræ, that nature has only to heal the wounds, and repair the loſs of the pieces. This I have myſelf beheld; and ſhall relate but one inſtance.

A ſlice about an inch long, and two lines broad, was cut from the tail of a large newt, 7 Auguſt 1778. The wound immediately healed; and from the 15, I ſaw a thin tranſparent ſlice all along the length, appropriated to replace the part taken away. In a few weeks, reproduction was complete, and the regenerated portion could not be diſtinguiſhed from the reſt of the tail.

Reflections.—If theſe experiments are compared with thoſe of the preceding Memoir, it is evident that they all coincide in confirming the reſults and concluſions relative to many phyſiological facts found in nature. Among theſe is the pre-exiſtence of germs deſtined to repair loſt members.

When, with the view of explaining this regeneration, we recur to the *powers of relation*, an *expanſive* or *vegetative power*, an *eſſential force*, *internal moulds*, and *organic molecules*, we certain-

ly uſe very ſcientific expreſſions, but to which no diſtinct idea can be attached (1). It is not enough to ſay, an effect is produced by a certain power: it muſt be demonſtrated how we conceive the effect depends on the power: and, by admitting the exiſtence of the cauſe, to give a ſatisfactory explanation of the principal peculiarities which the effect preſents. Now, when we ſay a leg or an arm is produced by the *vegetative*, or any other power, who can be ſenſible how the exiſtence of the limb naturally ariſes from ſuppoſing the power? Phyſical powers do not preſent their own limits: for any power is in itſelf indeterminate. How could the imaginary vegetative power determine the production of a leg rather than an arm, which it might juſt as readily

(1) The *powers of relation* were conceived by M. de Maupertuis. His ingenious work, *La Venus Phyſique*, muſt be conſulted for underſtanding how he uſes them in explaining the myſteries of generation.

The *vegetative* or *expanſive power* was created by Mr Needham.

M. Wolf, Profeſſor of Anatomy in Peterſburgh, endeavoured to introduce the *eſſential force* into organic nature. An account of his hypotheſis may be ſeen, Art. 334, *Corps Organiſés*, Note.

Organic Molecules are M. de Buffon's favourite hypotheſis.

readily produce? How could it give each part of the leg that figure, proportion, and ſtructure, which conſtitute the member? How could it arrange all the parts in that poſition and relation, in virtue of which all conſpire to the ſame end?

To affirm that a certain expanſive force, reſiding in the trunk of the old member, extends the veſſels, nerves, muſcles, and bones, is ſaying nothing at all: for it is evident, that only a ſimple ſtump or fleſhy cone would reſult from ſuch elongation. But could the cone have a hand or a foot at the extremity provided with fingers and toes? Could all their articulations be preſent; and could the ſame order and proportions prevail? Let imagination extend all the fibres of a bony ſubſtance, nothing will ever enſue but a ſimple oſſeous cone, ſo much the larger as imagination has rendered the force more powerful, and the expanding fibres more ductile. Could the extremity be faſhioned into a certain articulation? or how could an articulation, which infers certain figures, and theſe frequently very intricate, ariſe from the ſimple elongation of ſtraight or nearly parallel fibres? How could an expanſive power change their direction, and diſpoſe it ſo as to produce a joint, and alſo produce thoſe glands in the joint which are to lubricate its parts?

What

What I now obſerve of the oſſeous parts obviouſly applies to thoſe that are ſoft. How, for example, could extenſion of the fleſhy fibres of a newt's tail produce that infinity of glands that cover it; and ſecrete the viſcous fluid by which it is lubricated? How could extenſion of the fleſhy fibres, at the root of a ſnail's horn, produce a new horn, and place an eye at its extremity, provided with an uvea and three humours (1)?

I ſhall omit the *eſſential force*, which a very modern Epigeneſiſt has gratuitouſly aſcribed to matter. It would be equivalent to admitting the *plaſtic nature* and *creative mind* of Redi and Hartſœcker. Beſides, the ſame objections may be made to it as to the *expanſive power*. Theſe are expreſſions which enrich dictionaries, but add nothing to phyſiology. Here I muſt refer to Part 11 of *la Palingeneſie*.

Nothing need be ſaid on *organic molecules*, as an excellent obſerver has rigorouſly demonſtrated their non-exiſtence, by a beautiful ſeries of experiments

(1) It is to Swammerdam's amazing dexterity in diſſection that we owe our knowledge of the ſtructure of the eye of the ſnail. *Vide* his excellent hiſtory of the animal in his Biblia Naturæ, of which there is a French tranſlation. *Collection Academique*, Tom. 5.

periments and obſervations very much diverſified.

With regard to *internal moulds*, I think as unfavourably of them as of *plaſtic forces*. It muſt be remarked, that what are called moulds, are not ſimple cylinders or ſolid cones, but hollow tubes, nice textures, and the like. But, independent of this difficulty, where could be the internal mould of a hand, a foot, an eye, or brain, which no longer exiſt in the animal, but will nevertheleſs be completely reproduced? Do not let us confound the ſecretory organs with moulds; to ſpeak correctly, they form nothing; they only ſeparate certain molecules for certain ends.

Thus, becauſe I am unacquainted with any explanation merely mechanical, that will account for the leading phenomena, I admit, that in the interior of the members of the newt, there are germs deſtined to repair the loſt parts.

My experiments ſeem to prove, that there are different claſſes of theſe germs, and that they are not fortuitouſly diſperſed in the interior of the members, but diſtributed in regular order, relative to the different poſſible privations.

That there are various ſpecies of germs, ſeems to be aſcertained, from attending to what enſues in the reproduction of the hand and foot, compared with what ſucceeds in reproductions of the fingers

fingers and toes. A conical papilla becomes perceptible after amputation of the hand, which continues to encreaſe. Two fingers appear at the ſummit, then three, and at laſt four. The papilla, in its origin, was the germ; therefore, the germ contained the four fingers and all their concomitants. But ſhould one, two, or three fingers be amputated, one, two, or three conical papilla, much ſmaller, will ariſe, and gradually aſſume the figure peculiar to fingers. Therefore, theſe ſmaller papillæ were originally ſmaller germs than thoſe containing the whole member, and each contains only a ſingle finger. But this is ſpeaking incorrectly; they do not properly contain a finger; they are the finger itſelf, contracted and concentrated into the ſmalleſt poſſible ſpace. It is the ſame with the reparatory germs of the hands and feet; they are themſelves hands and feet in extreme concentration. But the arms, thighs, and legs, are equally reproduced; therefore, there are germs which do not include the hands and feet alone, but alſo an arm or a leg.

In the ſame way, do I admit reparatory germs for replacing joints; for I cannot conceive the ſimple mechanical formation of a joint, more than of a whole finger or hand.

What I obſerve with reſpect to the reproductions of newts, ſhould apply to thoſe of ſnails.

We

We have only to reflect on all that is said of the regenerated horns and eyes, and attend to their admirable organization.

It is evident, that we need not recur to germs, properly so called, for explaining the reproduction of a cutaneous or muscular fragment. In the skin and muscles, are numerous fibrilli, appropriated to repair these parts, and they unfold only when certain accidents direct the nutritive juices towards the reparatory fibrilli around the edge of the wound. Large animals, and even mankind, present many remarkable instances of similar reparations that succeed in the osseous parts.

In another place, I have explained myself at length on the various kinds of animal reproduction. There I have ascertained the different meaning that may be applied to *germs*, and explained the principles which seem most applicable to each species of reproduction (1). The enlightened and philosophic reader will decide between these principles and those which are opposed by our modern Epigenesists. Undoubtedly, he will not object the numerous germs that never unfold in newts, and other animals which repair their lost members, for he is not ignorant that the SUPREME WISDOM has proceeded

(1) *Palingenesie*, Part 10.

ed on a general plan in calling the different families of organized beings into exiſtence, or on plans that may comprehend all the individuals of each family. How many millions of ſeeds, how many millions of eggs produce nothing, yet every egg and every ſeed includes a minute organic whole, which never unfolds, though deſtined for evolution. The philoſopher will not haſtily conclude, that the exiſtence of theſe organic wholes is uſeleſs, becauſe it muſt inſtantly occur to him, that his knowledge is not ſo great as to diſcover all the uſe of beings, and becauſe he will eaſily conceive, that what is not appropriated to its particular uſe in this world, may be ſo in the next (1).

Experiments on newts, ſnails, earth worms, and the like, ſeem to indicate, that the original and primitive figure of germs is ſpherical or elliptical; at leaſt, this appears to reſult from the figure in which the members are firſt ſeen. In the beginning, they are very minute roundiſh buds, which gradually aſſume another ſhape that removes them farther and farther from their original one. The wonderful metamorphoſis of the chicken, before it attains its perfect ſtate, may enable us to judge of thoſe which the limbs of newts and other regenerating animals undergo;

(1) Conſult, Part 1, 2, 3, 4, 5, of the *Palingeneſie*.

go, before they appear fully evolved (1). But we have no means of discovering organic wholes so minute, and of contemplating their successive evolutions.

Finally, the experiments on newts demonstrate, that the germs of different orders are not fortuitously dispersed in the interior of the animals, but each placed relative to the situation of the member whose loss it is to repair. Thus an arm, or a leg, never unfolds, where there should be a hand or a foot; nor do we ever see a hand originate, where a finger only is defective. This is proved, by simple inspection of the figures, and of 4, 5, 6, in particular.

I cannot think the reparatory germs are deposited in the osseous parts, or in those that may ossify; I rather suppose they are lodged in the soft parts, as more favourable to their evolution. While a germ unfolds all its parts, engraft, or anastomose with the corresponding parts of the original whole, and form one body with it; this union is evinced by the reparation of the mutilated member, since the new part exactly resembles it. Something analogous to vegetable grafts takes place, as M. Duhamel has well described.

The

(2) Vide *Considerations sur les Corps Organisés*, chap. 9. Haller, *Memoire sur le Poulet*,

The reproducing members of the newt and ſnail illuſtrate, that organic wholes may be completely developed without fecundation, in its proper ſenſe. The moſt ſubtile or active fluids of the animal are ſufficient to effect their evolution. Nothing need here be added to what is ſaid on the ſubject, *Contemplation de la Nature*, *Chap.* 3. *Part* 10.

Although the ſame member is mutilated five or ſix times ſucceſſively, it will be regenerated as often. Probably theſe ſucceſſive reproductions extend further; but we are yet ignorant of their limits. Theſe can be diſcovered by experiment alone; but it is evident that they cannot be infinite. In the ſame freſh water worm, I formerly ſaw twelve ſucceſſive reparations (1).

The

(1) *Traite d'Inſectologie*. 'It is very probable, that the 'property of theſe inſects to renew a head or tail, when 'deprived of either, is proportioned to the number and 'nature of the accidents to which they are expoſed.'

In rivulets, I have ſometimes found worms that had loſt the head or tail, and then repaired it. Snails run the hazard of ſimilar accidents. Muller, at the end of his memoir on ſnails, *Journal de Phyſique* 1778, obſerves, that he found 'a naked ſnail in a wood, and two others, repairing a 'large horn that had accidentally been loſt.' I have had newts taken with deformities in the fingers and toes, which clearly indicated caſual mutilations.

3.
n

2.
r
1
2
3
4

Fig. 1.
r
s
1
2
3
4

6.
n

5.
n

4.

9.
b

8.
r
a
b
c
d
e

7.
n
n

10.
c

11.
b

12.
c

The members in miniature, which, though themſelves mutilated, produce a ſimilar, but ſmaller miniature, and the reproductions of this, when mutilated, afford a powerful ſupport of the hypotheſis of involution. However, I will not affirm that the reparatory germs are involved in each other; the expreſſion would be incorrect; but it appears that the expanding germ includes all the parts fit to reproduce a member, and, with theſe parts, germs united to them, which alſo grow, and that by their connection, and are deſtined to replace the loſt members. In ſhort, different generations of germs muſt not be conceived as boxes caſed in each other. The germ of the ſecond generation conſtitutes part of the germ of the firſt, juſt as a ſeed growing in a plant, or an egg in an animal, forms part of that plant or animal. The germ of the third generation conſtitutes part of the ſecond, and ſo on with the reſt. Thus all the generations included in the firſt germ are as many decreaſing parts of it, and it is itſelf a conſtituent part of the original member. I need not return to the objections againſt involution; they are anſwered elſewhere; and none ſimilar will occur to a philoſopher who knows the difference between hypotheſis and diſcovery.

MEMOIR III.

EXPERIMENT I. *Reproduced members mutilated.*—In the firſt experiment of the preceding Memoir, is an account of ſeveral ſucceſſive operations on a large newt, which were made purpoſely to diſcover whether reproduced members contained reparatory principles in the ſame way as the original members. There, it is obſerved, that the fourth mutilation was performed 13 October 1778. Another operation was performed on the ſame newt, 9 March 1779; the reproduced hand and foot being nearly as much advanced as thoſe the laſt time amputated.

We have ſeen that the third amputation was on the 24 of Auguſt, ſo that only fifty days elapſed between it and the fourth; whereas, between the fourth and fifth, there were one hundred and forty-ſeven, which is an additional proof how much reproduction is retarded by winter.

A new hand was announced by the appearance of new fingers, 21 April, and regeneration of the foot was at that time ſtill more ſenſible.

Three

Three toes could clearly be diſtinguiſhed, notwithſtanding their extreme minuteneſs. On the ſixth of May, only three fingers were viſible, but all well ſhaped; the longeſt equal to about two-thirds of a line. The three toes were nearly of the ſame length, fig. 1. 2. plate 11. The extremity of the arm and the foot are much ſwelled, fig. 1. 2. *r*.

It is here to be remarked, that neither fingers nor toes are in the ſame direction as the axis of the member, but, from being a little elevated, they are oblique, which is more conſpicuous in the latter.

June 1, the hand was exactly as in fig. 3. Four fingers were completely formed, the longeſt near a line and a half in length. The foot ſtill had only three toes, nearly as long as the fingers, fig. 4. I this day performed the ſixth operation, without waiting the reproduction of the other two toes, which ſeemed very tardy.

A new hand was viſible in the end of the month with three fingers, and a foot with four toes.

July 14, the hand ſtill had no more than three fingers, and the foot four toes; the longeſt toe about a line and a third; the largeſt finger a little leſs in length. It is almoſt unneceſſary to repeat, that all were whitiſh or greyiſh, and ſome tranſparent, as originating members uniformly

are. The toes had grown greatly againſt the 31, and darkened very much, but the increment of the fingers was leſs evident, nor was their colour ſo dark. Neither the fourth finger nor fifth toe yet appeared.

The ſeventh operation was now performed; and a remarkable monſtroſity aroſe on the reproduced foot, fig. 6. 15 October.

The foot ſeemed to have renewed only three toes, and the ſecond, *m*, was monſtrous. It was evidently formed by the union of two; one-third of the length was thicker than the reſt, and a cleft ſeen at the upper extremity, where the toes were not united.

The firſt and fourth finger were extremely ſhort, fig. 5; the ſecond and third more diſtant than they ſhould have been, and the whole hand turned downwards, which increaſed the deformity.

I proceeded to the eighth mutilation, 29 October, no ſenſible change having taken place in the members ſince the 15, except in the colour becoming deeper.

While the amputated foot lay on my finger, I diſtinctly ſaw with my naked eye, and ſtill better with a magnifier, a ſlight furrow or groove, all along the monſtrous toe, and exactly in the middle, which ſeemed an indication of the place where the two toes engrafted or united, and pro-

duced

duced the monſtroſity. It is magnified, fig. 7, and the furrow, or groove uniting the toes, is plainly evident.

This is a moſt important fact, as it relates to the celebrated diſpute concerning the origin of monſters. Here we behold a graft by union, which nature has executed in a manner before the obſerver. The large horns of ſnails have preſented ſimilar inſtances, as appears from the horn, fig. 15, Plate 8. which aroſe from ſuch a graft, and it is proved by the two eyes at the extremity. But as the horns of a ſnail are originally ſofter than the members of a newt, and contain no oſſeous part, or what may oſſify, it naturally follows, that no indications of the junction will remain.

To my great regret, the newt died 27 November, probably from the bad quality of the renewed water. At that time, the animal had not exhibited any ſign of reproduction.

EXPERIMENT II. *A ſmall excreſcence cut from the leg of two newts.*—On the 28 of October 1778, I remarked that one of the large newts, which had only four toes on a foot, had, in place of the fifth, a very minute excreſcence, apparently an originating toe. However, I could not obſerve that it made any ſenſible progreſs; and I determined to cut it off, with the view of diſ-

covering whether nature would produce ſomething in its place, or regenerate the fifth toe. But nothing reſulted from the operation.

A ſimilar experiment was alſo made on another newt, and with as little ſucceſs. Undoubtedly the excreſcence was not the germ of a toe whoſe evolution ceaſed. It probably aroſe from ſome accidental cauſe: and as it did not include the germ of a toe, the exertions of nature were limited to ſimply healing the wound.

EXPERIMENT III. *On a monſtrous finger.*—On the 13 of July 1779, my newt catcher brought me one with the ſecond finger of the left hand monſtrous. There was a cleft at the extremity ſimilar to what had been obſerved in the newt of the firſt experiment, which alſo ſeemed to be formed by the accidental union of two fingers ingrafted four-fifths of their length by approximation.

Three days afterwards, the hand was drawn of its natural ſize, fig. 8. Neither it nor the fingers appeared to have been fortuitouſly mutilated: they were of the ſize, colour, and proportion peculiar to a hand fully developed, that had never been expoſed to any accident. Therefore the newt was probably produced with the monſtroſity. In the finger *m*, which is evidently larger

er than the reſt, is obſcurely ſeen the little groove, indicating the place of union. A wide angle is formed by the cleft, *b*, *b*.

While conſidering this ſpecies of monſtroſity, an experiment occurred to me for perfecting the theory of germs. I cut off one of the branches cloſe to its origin with very ſmall ſharp pointed ſciſſars, 3 July.

On the 19 of Auguſt, the finger had protruded a new branch half as long as the correſponding one, and evidently thicker: and on the 25, it was equal in length, but ſtill thicker.

This day, I cut off the unmutilated branch at its baſe. Reproduction advanced much ſlower; and not before the middle of October was it equal to the part replaced.

Both branches were amputated on the 21. My object was to diſcover whether the finger would reproduce a cleft ſimilar to the firſt: but an accident which befel the newt prevented the ſatisfaction of my curioſity.

However, it is proved by this experiment, that each branch contained a principle of reproduction, the evolution of which was determined by the operation.

I afterwards procured another newt with the ſame monſtroſity of the third finger. It was caught during ſpring of the preſent year 1780. On the 1 of September, I amputated the cleft at

its origin: and a new one appeared on the 28. Thus each could be reproduced separately, and both at once.

Experiment IV. *Both hands and the left foot longitudinally divided.*—The experiments on longitudinal division, which afforded interesting results, are detailed in the preceding Memoir: It was of great importance to repeat one of this nature; for which several reasons have already been given. And I may here add another. By this mode of section, I thought a greater number of fingers and toes might be produced than nature allotted to each member. I conceived that longitudinal division might occasion the derivation of fluids favourable to the eruption of those germs that were around the edges of the wound. Thus it was principally with that view the following experiments were made.

July 13, 1779, the hands and left foot of a newt were longitudinally divided. From the 15, mould grew on the wounds, but I took care to brush it off with a pencil: and also to renew the water frequently. But, for all my precautions, half the hands and foot were consumed by gangrene. A minute trunk, formed by the metacarpus and metatarsus, was then visible at one side of the remaining part.

I never doubted that the trunk would produce new fingers and toes: my former experiments had prepared me for it: and in fact, I did see little papillæ on each trunk after an interval of some weeks, which announced the reproduction of parts in the place of those destroyed. The first two fingers of each hand were wanting, and the last three toes.

Two new fingers, about two-thirds of a line long, and in the natural position, appeared on each hand, 21 August. The second of the right hand exhibited a very singular monstrosity of the species already described. This finger was much larger than it should have been, and evidently arose from two germs grafted by approach. An originating cleft, similar to the others, was perceptible at the extremity.

The hand is magnified, fig. 9. *m* the monstrous finger: *d*, the other beginning to grow. The foot is of its natural size, but the originating toes, *n*, *n*, in an unnatural position, fig. 10.

Though the monstrous finger was examined with a considerable magnifier, nothing along the middle, which indicated the union of the two fingers, could be discovered: all was uniform. This, as well as the whole new fingers and toes, was semi-transparent, and of a whitish or greyish colour.

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EXPE-

EXPERIMENT V. *The hands and feet of a newt longitudinally divided.*—On the 15 of July 1779, I divided the hands and feet of a large newt longitudinally. Mould soon appeared on the wounds: but by removing it with a pencil, and renewing the water, I saved the members.

The halves of the right hand and foot re-united: and the enormous wounds were so completely cicatrised, that it was impossible to observe the place of junction, at least in the hands: however, I thought I could perceive a long superficial furrow which might be it.

The left foot had lost the middle toe by the operation. But in a few weeks, there was discovered, in the middle of the foot, an extremely minute papilla among the four toes, which announced a new toe to supply the place of that destroyed.

I soon observed a similar papilla among the fingers of the left hand, which was recognised as a *medius*, unfolding exactly where the middle finger is in a hand with five fingers. But newts have only four; therefore, by a longitudinal section, this newt acquired five on one hand, and my object was happily accomplished.

The new toe was a full line in length; the new finger only half a line. Both the hand and foot were then designed, fig. 11, 12. In consequence of dividing the hand, the second finger is farther from

from the third than uſual, fig. 11. Between them is a middle finger ſtill very ſmall, but evidently a new one, *m*. From the ſame cauſe, the ſecond toe is alſo farther from the third than it ſhould be. Between them is ſeen the new toe, *n*, which is to replace that which was loſt.

The wound on the metatarſus was now ſo well healed, that no marks of it remained. Theſe immenſe wounds being healed by a kind of ingrafting, which effects the union of the parts, is no inconſiderable corroboration of a theory which I have ſuggeſted on the formation of certain monſters, that ſeem to originate from the accidental union of two organic wholes. Nor is the ſupernumerary finger, which I was able to give the newt, leſs favourable to it.

Experiment VI.—We muſt not conclude, from the experiments in this and the preceding memoir, that, in conſequence of longitudinal diviſion, a new finger will always ariſe in the middle of the member, and one, in this manner, have it in his power to produce a fifth finger on hands, and a ſixth toe on feet, at pleaſure. Experience has taught me, that cleaving the members aſunder is not uniformly followed by an additional reproduction. Many circumſtances, which we are yet unable to aſcertain, may deeply influ-

ence

ence the effects of the operation; and of these, the following is an example.

By a cut as far as the tarsus and carpus, I divided the hands and feet of a large newt longitudinally, 25 August 1779. After the operation, the parts separated far asunder. I expected much mould on the wounds, but so little appeared that it was unnecessary to remove it. The middle toe of the left foot was destroyed.

In about six weeks, no traces of the wounds remained. The third and fourth finger of the left hand, and the third and fourth toe of the left foot, were more distant from each other than naturally. The middle toe of this foot was replaced by another, which was about two lines long, 29 October.

Experiment VII. *Various monstrosities arising from the amputation of members.*—Here I shall subjoin some remarkable instances of monstrosity arising from the amputation of members.

A foot which had produced six toes, but three of them united great part of their length, is represented, fig. 13. The same foot is magnified, fig. 14, to shew the monstrosity better. On comparing this with fig. 8, 9, the monstrous toe evidently exceeds the natural size, was it not formed as the others by the union of two. Here there are two clefts instead of one. The branch

a, is longer than *b*, *c*: the middle one *b* is ſhorteſt. No indication of the union was perceptible, it was ſo complete.

Another newt, whoſe hands and feet were tranſverſely amputated, 8 June 1780, preſented a monſtroſity ſtill more ſingular. The four members began to reproduce in the end of July. Conſidering the newt attentively, towards the firſt days of Auguſt, I was agreeably ſurpriſed to ſee eight toes regenerating on the left foot. It was neceſſary to hold the foot in a particular poſition and examine it cloſely, as they could not be diſcerned at the firſt view.

This monſtrous foot was deſigned Auguſt 9. The metatarſus, *m*, is a little larger than common; and the fifth toe, *c*, ſeems to riſe above the fourth, *p*, fig. 15.

There was alſo a ſingularity in the right hand. It reproduced five fingers; the firſt two much ſhorter than the reſt. It was deſigned 10 Auguſt, fig. 16.

The regenerated right foot was likewiſe deformed; the laſt two toes being connected by a membrane, like that of ſome aquatic animals. Thus the only perfect part of the newt was its left hand.

Again obſerving the monſtrous foot, fig. 15, with the greateſt attention, November 7, I diſcovered that the fourth toe, fig. 17, 5, 6, 7, was

compoſed

compoſed of three longitudinally united, and nearly as in fig. 14. The third toe, alſo, was clearly formed by the union of two, but joined only a ſmall part of the length. All this gave the reproduced foot a very uncommon appearance, and occaſioned a confuſion, which prevented the figure and arrangement of the parts from being eaſily underſtood.

Is it not indicated, by theſe frequent graftings, that the reparatory germs of mutilated parts are ſituated very near each other, in the interior of the members, becauſe they can only ariſe in conſequence of ſuch proximity? Such remarkable inſtances enable us to judge how many operations may be performed on newts, well calculated to elucidate the myſterious origin of monſters. It is proved that reproducing members, if mutilated, produce ſimilar members. Thus may various monſtroſities be regenerated. If the eight toes of the monſtrous foot were amputated, which is an experiment that I mean to make, eight toes reſembling the firſt would moſt probably be reproduced; and reproduction might perhaps extend further, by encroaching a little on the metatarſus.

EXPERIMENT VIII. *The limbs of a newt diſlocated.*—Sig. Spallanzani having requeſted me to diſlocate

diſlocate the limbs, I did not fail to make this experiment. But it is much more difficult to diſlocate a limb than to amputate it; for the pliancy and lubricity of the members contributes to render the experimentaliſt's exertions abortive.

On the 13 of July, I performed this new operation on two large newts, at half paſt two. It is of ſome conſequence to attend to the exact hour. Both arms of the one and both thighs of the other were diſlocated. The operation was indubitably complete; for, beſides the ſenſation felt in performing it, which convinced me of the fact, the members immediately afterwards were pendent, as if dead, and the animal had no longer any power over them, which was an unequivocal proof.

At ſix in the morning, on the 14, it was impoſſible to recogniſe any ſymptom of diſlocation. In the evening, each newt moved the disjointed limbs with a liberty and facility which announced that nature had already repaired the diſorder.

EXPERIMENT IX. *On the eyes of newts.*—This and the preceding experiment are cruel indeed; and ſenſible minds will hardly pardon the obſerver's cruelty, though it ariſes from an evident

dent desire to discover new facts and enlarge our knowledge of the animal œconomy. Therefore, I fear the compassionate reader will revolt further still at what is yet to be related. But I beg he will consider, that animals, which, after losing one, or even several limbs, continue greedily devouring the prey presented, undoubtedly cannot experience the sensation of pain to the excess which our own sensations lead us to imagine. We are very insufficient judges of what passes within an animal so remote from us in the scale of living beings. Let it not be thought, that by these reflections, I mean to lessen the natural repugnance of every humane mind to make animals suffer. The benignity of nature itself will inspire man with this precious sentiment to prevent the enormous abuse that his power might exert over the animals which she has subjected to his dominion. Yet let me ask, whether a rational person abuses his empire over animals, by making them suffer only for his own instruction, or that of his fellow creatures.

With a scalpel, I extracted the right eye of a large newt, 13 September 1779: but I did not obtain the globe without much injury to the tunics. It was the first time of performing the operation, and before I had acquired the peculiar dexterity necessary for success, and afterwards learned

learned by experience. Thus the utmoſt diſorder enſued in the eye, and the chryſtalline lens ſtarted out on my nail. This is a beautiful object; no larger than a millet ſeed, and quite tranſparent. I thought that I beheld one of the ſpherical lenſes with which Leeuwenhoeck diſcovered ſo many wonders. But contact of the air ſoon tarniſhed the minute lens; it dried and became disfigured.

A deep bloody wound in the ſocket of the eye was the conſequence of this cruel operation. And the reader will not be ſurpriſed if I hardly expected any thing from it, and that the newt would probably remain blind for ever. How great was my aſtoniſhment, therefore, when, on the thirty-firſt of May 1780, I ſaw a new eye formed by nature. The iris and cornea were already well ſhaped, but the latter wanted its peculiar tranſparency, which is very conſiderable in theſe animals. Impatience to arrive at the moſt important part of the prodigy has induced me to omit the progreſs of it; and obſerve that nature certainly began with cloſing the wound.

The eye was completely repaired 1 September. The cornea was nearly as tranſparent as that of the other eye, with which it was frequently compared. The iris had alſo acquired the yellow gilded colour, which characteriſes this ſpecies of newt. In ſhort, the eye was ſo per-

fectly renewed, that no veſtige exiſted of the uncommon operation that the animal had undergone. During the remainder of this and the following month, the cornea always became more tranſparent; and now, when I write theſe remarks, 8 November 1780, it is equally perfect as the other: but the reproduced eye ſeems a little ſmaller than the entire one; and the iris, or golden circle, goes only half round the ball (1).

It would ſtill be neceſſary to extract the reproduced eye, to aſcertain, by diſſection, whether it contains a chryſtalline lens ſimilar to the original. But I confeſs, that, as yet, I have not had reſolution to ſubject the newt to the moſt barbarous of all operations; and I ſhall probably await its death for ſatisfying my curioſity.

A newt is mentioned in the ſecond memoir, which had loſt the regenerated members four times, and had taken no food for ſeveral months. It became ſo much emaciated, that the joints of the fingers could be ſeen through the fleſh. The head

(1) According to Blumenbach's obſervations, this experiment has failed when the whole globe of the eye was extracted. In ſix months, after diſcharging the humours, and cutting out the tunics, the eye was reproduced. Even after eleven months had elapſed, it was not ſo large as the other, *Specimen Phyſiologiæ Comparatæ.*—T.

Fig. 1.
r
1.
2.
r
3.
4.
5.
6.
m
7.
s
8.
m
b
b
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12.
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c b a
15.
m
p c
16.
17.
1
2
3
4
5
6
7
8
18.
n
p
3
c c

head had ſhrunk in ſuch a manner, that the eyes projected like two lumps, and appeared much larger than uſual. It died 11 November; and I extracted an eye. The opaque cornea or ſclerotides, *o*, fig. 18, plate 11, is of an iron grey; *p*, the ball; *c*, the tranſparent cornea; *n*, the optic nerve. The eye is ſhaped like a flattened ſpheroid, the edges of the ſclerotides forming the equator. But my purpoſe is not to deſcribe this organ.

ADDITIONAL

NOTES AND OBSERVATIONS

BY THE

TRANSLATOR.

INTRODUCTION, p. 13.—Truſting to Linnæus, it is ſaid the tortoiſe lays 1000 eggs. I ſhould rather ſuppoſe that learned naturaliſt is correct, but I have found no authority which makes the animal nearly ſo prolific. However, there is a great difference in the numbers which the females of very fertile animals will produce. I have in ſome obſerved intermediate numbers from fifty to a thouſand eggs.

TRACTS, vol. 1. p. 50.—The author's neglect, or rather contempt of nomenclature, occaſions conſiderable difficulties to thoſe who ſtudy his

works. Indeed, all the ſyſtems we have are ſo brief and indiſtinct, that even the moſt expert naturaliſt will ſometimes be at a loſs to diſcover whether the animal there named is truly the object of his ſearch.

It is the *Oeſtri* that depoſit their eggs in the ſkin of cattle; and great pain is occaſioned to the animal by the ſwelling of tumours, which include the larva of the inſect. *Aſili* are a different genus of flies.

P. 84.—Since the note on the torpidity of fiſhes, I obſerve that the fact is confirmed by late obſervations, which will be found in CEPEDE *Hiſtoire Naturelle des Poiſſons*, Tom. 3. Paris 1802. 'Formerly we had vague ideas concern-'ing mackrel in their ſub-marine aſylums during 'winter, and particularly in the polar regions; 'but we are now indebted to vice Admiral Ple-'ville le Peley, for certain information. He has 'verified his obſervations along the coaſt of 'Greenland, Hudſon's Bay, and towards the 'banks of Newfoundland. In theſe northern 'countries are arms of the ſea, named *barachoas*, which are ſo interſected by points croſſing 'them, that at all times the water is as calm as 'in a baſin. The depth of theſe places is va-'rious,

'rious, according to the proximity of the banks, 'and the bottom is generally covered with ſoft 'clay and marine plants. It is here that the 'mackrel lie in concealment during winter; and, 'thruſting the head and anterior part of the bo- 'dy about a *decimetre* into the mud, their tails 'are kept perpendicularly above it. Thouſands 'are found thus buried in each *barachoua*, and 'cover the bottom of the baſin in ſuch a man- 'ner, that ſeamen, unacquainted with the coaſt, 'have been afraid to approach what they thought 'were ſhoals. In Citizen Pleville's opinion, the 'ſurface of theſe *barachouas* is frozen in winter; 'and the thickneſs of the ice, as well as the ſnow 'above it, moderate the effects of the ſeaſon, and 'contribute to preſerve the animals alive. Only 'towards July do the mackrel recover part of 'their activity, depart from their holes, and, com- 'mitting themſelves to the waves, traverſe the 'great banks. It even appears that this ſtupor, 'or torpidity, is diſſipated by degrees. Their 'ſenſes are feeble for twenty days; their ſight is 'ſo weak, that they ſeem blind; and they are 'eaſily caught in nets; afterwards they can only 'be taken with the hook, but being emaciated 'from long abſtinence, and greedy for the bait, 'great numbers are caught,' page 32, 33.

P. 207.—Though M. Bonnet is of opinion that the incorporation of foreign particles removes the original tranſparence of animals, it is probably owing as much to chemical combinations.

Seminal Vermiculi.—It is yet uncertain by whom theſe animals were firſt diſcovered. Different authors aſcribe the diſcovery to various naturaliſts who lived towards the end of the ſeventeenth and in the beginning of the eighteenth century. However, it is undoubted, that Leeuwenhoeck was the firſt who inveſtigated their properties.

It was generally believed, that all ſeminal vermiculi were of the ſame ſize; that thoſe of the frog were as large as the vermiculi of the horſe. The author has now put it beyond diſpute, that there is not only a difference in the ſize of thoſe of different animals, but that all the vermiculi of the ſame animal are not equally large.

Vol. 2. *Wheel Animal.*—This animal I have often found in ſituations without the ſmalleſt particle of ſand: indeed, it has repeatedly appeared in different infuſions, and in great numbers. But I

do

do not recollect to have met with it in any liquid with a tendency to putrefaction.

The horn of the wheel animal is situated on the upper part, and is not easily observed unless when the animal is just about to make its step.

P. 163, Note.—Before terminating this work, it is proper to correct what is probably an error. It is possible, that what I conceive a new species of *sloth* is not so. Some points in its history have already been attended to by naturalists of great eminence.

In the French translation of *Fuesli's Archives of the History of Insects*, Winterthour 1794, there is a memoir on these minute animals by the celebrated Muller. His observations coincide almost exactly with mine; and he seems to have had the advantage of a number for examination.

He observes, in the first place, that 'Eichorn 'and Goetze at the same time discovered the ani-'mal, but the former ascribed ten feet to it instead 'of eight; and the name they bestowed upon it 'was the URSLET (*Ourslet ou Petit ours*), from 'the supposed resemblance to a bear.'

In

In Muller's opinion, it is not an infuſion animalcule, 'though its proper abode is among 'them, on the water lentil. The figure and 'number of feet approximate it to the genus of 'mites or acari; and although neither eyes nor 'antennæ are perceptible, the other parts induce 'us to claſs it with inſects.'

He proceeds: 'Three claws terminated each 'foot; ſomething like eggs were ſeen within. 'Sometimes I found a ſimple ſkin with the feet, 'nails, and eggs. This is not a dead *urſlet*, but 'a ſlough, ſuch as other animals throw off; but 'how does the ovary come here?'

The figure in the Archives, Plate 36, ſeems very correct. If we may judge from what is ſaid in the account of it, this animalcule muſt have been a great deal larger than mine.

Whether it belongs to the claſs of inſects may be doubted: if it does, I ſee few reaſons why Spallanzani's does not belong to it alſo.---It is very probable, that ſeveral of what we call *infuſion animalcula*, are only miniatures of ſome larger animals.---Thoſe of the northern nations have alone been inveſtigated; it is reaſonable to expect new animalcula as well as we find new animals

mals in other countries, or new connections with those already known.

Boat Worm.—I am uncertain what animal, according to the Linnæan nomenclature, the author calls the boat worm. The third chapter of his *Prodromo* is appropriated to its reproductions; and he thus describes the animal itself. 'It is 'annulated like the earth worm, by which means 'it can contract, extend, and move from one 'place to another. Towards the head, it is 'as thick as a large goose quill. It is generally 'about eight inches long, but the largest, when 'extended, are a foot. The colour of the back 'is a mixture of grey and brown, which becomes 'lighter towards the tail: the belly is whitish.

'The ordinary abode of this animal is in clear, 'shallow, or gentle running water. The anterior 'part is buried in the mud, but the posterior rises 'to the surface of the water. There it enlarges 'and curves below like a boat which extends on 'the surface. The concave part of the boat is 'uppermost; and the rising of the edges above 'the water seldom allows it to flow over. Without this particular figure it would sink, as it is 'specifically heavier than water.—These peculiarities have induced me to call it the *boat worm.*'

The

The freſh water worms, ſo often mentioned, ſeem to be the *Lumbricus Variegatus*, and *Tubifex* of Linnæus. A full account of them will be found in the firſt volume, *Œuvres de Bonnet*. The ſea nettles are *Meduſæ*. But the reader will obſerve, that, throughout this work, the original names are preſerved, without uſing thoſe in the *Syſtema Naturæ*, from the hazard of error, and the ſame with reſpect to the different elaſtic fluids.

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FINIS.

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